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ORIGINAL LECTURES.

PHILADELPHIA COUNTY MEDICAL SOCIETY LECTURES.

ON THE PHYSICAL EXPLORATION OF THE LUNGS BY MEANS OF AUSCULTATION AND PERCUSSION.

A course of three lectures delivered by invitation before the Philadelphia County Medical Society.

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LECTURE II.

AUSCULTATION.

Delivered December 16, 1882.

THE exploration of the lungs by means of auscultation divides itself into the study: *First*, of the normal respiratory sounds and their abnormal modifications; *second*, of adventitious sounds, or râles; and, *third*, of vocal sounds, the latter including, with the loud or laryngeal, the whispered voice.

STETHOSCOPES.

The question whether auscultation should be mediate or immediate, is easily disposed of. It should be practised in both ways, according to the circumstances in particular cases. Laennec, as is well known, employed only mediate auscultation. He probably failed to recognize the value of immediate auscultation, because all his observations were made exclusively with the stethoscope. Some of those present may be interested in seeing the kind of stethoscope which Laennec employed. His first instrument was a cylinder of paper, consisting of three quires rolled together and kept in place by paste. He then tried instruments made of metal, glass, and wood, instead of the paper cylinder. Quoting his words, "In consequence of these various experiments, I now employ a cylinder of wood an inch and a half in diameter and a foot long, perforated longitudinally by a bore three lines wide, and hollowed out into a funnel shape to the depth of an inch and a half at one of its extremities. It is divided into two portions, partly for the convenience of carriage and partly to permit its being used of half the usual length. The instrument in this form—that is, with the funnel-shaped extremity—is used in exploring the respiration and the râles; when applied to the exploration of the heart and the voice, it is converted into a simple tube with thick sides, by inserting into its excavated extremity a stopper or plug traversed by a small aperture, and accurately adjusted to the excavation. This instrument I have denominated the *stethoscope*."

The stethoscope which I exhibit has an interest aside from the illustration of the kind which Laennec used. It not only belonged to Laennec himself, but was undoubtedly made with his own hands. Its authenticity, as having belonged to Laennec, is indubitable. It was given to me by a former colleague, the late Professor Choppin, of New Orleans, and it was given to him in Paris, by an old physician, who was Laennec's *interne* in the Hospital Necker, and who received it from Laennec himself. Laennec, as is known, was accustomed to make stethoscopes for his own use and for his friends. Moreover, the instrument bears intrinsic evidence of having been made by an amateur mechanic. When in the possession of the *interne* referred to (whose name I have forgotten), it was evidently not regarded with the same respect which it now claims, for the aural end has the traces of a penknife, showing that this sort of petty vandalism is not exclusively an American trait.

In connection with the stethoscope made by Laennec, I exhibit another, which is of interest, as having belonged to Valentine Mott, who obtained it in Paris, when the stethoscope devised by Laennec was in common use. This instrument, as you perceive, was made by a true mechanic. It is nicely polished and the ends are encircled with ivory.

In a recent lecture on the "Evolution of the Stethoscope," Samuel Wilks, of London, says, "I know not who invented the instruments with flexible tubes, but I have no doubt that a search into medical history would tell us." The stethoscope, with a single flexible tube, was devised by Carter Nestor Pennock. It was a great improvement on the wooden cylinders of every variety of shape devised by auscultators in different countries. Pennock's stethoscope consisted of a bell-shaped pectoral portion of metal, connected by a hollow flexible tube with a metallic ear-piece, the latter being introduced within the meatus auditorius. A still greater improvement was the binaural stethoscope devised by Cammann, in 1854. The advantages of this stethoscope are so great that, after a fair trial by any one, it is sure to supersede any other at present in use. After nearly thirty years from the date of its introduction by Cammann, it has come into considerable, but not as yet general, use throughout our own country, and it is but little used in other countries. I can speak of this instrument after ample experience, inasmuch as I have used it almost daily since 1855.

In my work on *Physical Exploration*, published in 1856, I stated that it was more difficult to judge of the quality and pitch of sound transmitted by Cammann's stethoscope, than with the wooden cylinder. This was an error, arising from my not then having used that stethoscope sufficiently to appreciate it fully. I corrected the error in a subsequent edition, but the error was quoted by Walshe, and appears in all the subsequent editions of his work on diseases of the lungs. Much to my regret, therefore, I may unwittingly have done something toward retarding the adoption of the instrument by our British brethren. Wilks, however, in his recent lecture, already referred to, appears to consider the binaural instrument as the result of the "evolution of the stethoscope" up to the present time, on the "principle of selection and the survival of the fittest," and he remarks that "the primitive instruments are indeed only to be found amongst the fossilized curiosities—the relics of former ages—on the antiquated shelves of some very old medical practitioner." The advantages of the binaural stethoscope relate to a conduction of sounds far better than by any uniaural instrument, and to greater facility in its employment. But there are certain obstacles to be overcome in the way of the appreciation of the first of these advantages. Want of knowledge of these obstacles is, I am persuaded, a reason for the fact that, after nearly thirty years, this stethoscope, except in some parts of our own country, is not in common use. In the first place, many stethoscopes, sold as the binaural stethoscope of Cammann, are essentially defective in their construction. Let me

indicate the points which are often overlooked by instrument makers. Cammann arranged the curves at the aural extremities so that when the terminating bulb is inserted into the ear, the opening in the bulb should have the direction of the external auditory canal. The conduction of sound is chiefly by the column of air within the instrument. This fact is readily demonstrated by obstructing one of the tubes leading to the pectoral end; the sound is prevented from reaching the ear on the side of the obstructed tube.

Another point in the construction is the size of the terminating bulb. This should not be too large to enter the meatus readily; and, on the other hand, if too small, passing too far into the meatus, it occasions discomfort and even pain. If these points be not properly attended to in the construction, the instrument is almost useless, and many instruments which have come under my notice have been defective therein. The flexible tubes should not be stiff. If they be so, every movement of the pectoral extremity acts within the ear as a lever, and occasions discomfort. The flexible tubes should move noiselessly. Sometimes the tubing used gives rise to a creaking sound which obscures other sounds. The elastic or the spring which is to hold the ends of the instrument within the ear should be neither too weak nor too strong. All these are minute points, but they are essential. It is proper to state that they are attended to by the instrument makers who made the first stethoscope under the personal direction of Cammann, Messrs. Tiemann & Co., and I have seen stethoscopes made by Ford, of New York, with which no fault could be found. I am afraid it is not safe to trust to the majority of instrument makers, and yet I hesitate to make a sweeping assertion of this kind.

Another obstacle applies to all perfectly constructed stethoscopes, namely, a humming sound belongs to the instrument, and this, for a time, confuses the attention. After a little use this obstacle disappears, the humming ceases to be observed, and the attention is free for the chest sounds. Before this, however, the instrument is often thrown aside as unsatisfactory. I have had much experience in giving practical instruction in auscultation to classes, and I have always found that at the commencement of a course most members of a class appreciate thoracic sounds better with the ear applied directly to the chest than by the use of the binaural stethoscope, but after a short time the stethoscope becomes so attractive that it is difficult to enforce sufficient exercise of the ear in immediate auscultation.

Wilks, in his interesting lecture on stethoscopes, refers to a fact first pointed out to him by Andrew Clark, which relates to a peculiarity of the binaural instrument in the objective appreciation of sounds. It is as follows: "If each ear-piece be separately used, and any sound be made near the mouth-piece, it is heard in the ear itself; but if the two pieces are employed together, the sound is heard at the spot where it is produced." "This fact," he adds, "corroborates the theory as to the value of a double set of senses, ... the two ears listening to the same sound more thoroughly appreciate its objectivity." This fact is easily verified.

I should, perhaps, refer to Alison's differential stethoscope, which I exhibit. It is a binaural stethoscope, but with a double pectoral extremity. Sounds from two situations are received simultaneously, but from each situation, into one ear. The theory is that the sounds from the two situations can in this way be best compared with each other. The theory is fallacious. If we wish to bring into comparison two sounds which are not musical, or if we wish to compare the quality of two musical notes, we do not desire to listen to both at the same instant, but to each in succession. The increased conduction of sounds by means of a binaural conductor is not obtained by Alison's stethoscope, and a little practice will render it evident that with Cammann's instrument sounds in different situations, listened to consecutively, are much better compared than by the so-called differential stethoscope.

THE NORMAL VESICULAR MURMUR OF RESPIRATION.

Auscultatory sounds, like those obtained by percussion, are to be studied analytically with reference to their differential characters. They are to be studied with special reference to differences in pitch, quality, and intensity; but the other points of distinction which have been mentioned already are to be considered; namely, the duration of respiratory sounds, the rhythmical succession of the sounds of inspiration and expiration, and the apparent nearness to or distance from the ear, especially of vocal sounds. It is needless to say that in the study of auscultation, as of percussion, the analysis of the normal sound should precede and be the point of departure for determining the differential characters of the abnormal sounds which are signs of disease. Entering upon the consideration of respiratory sounds, the normal pulmonary murmur of respiration is to be first considered.

Seeking naturally to give an idea of this murmur by likening it to something familiar, Laennec found no better comparison than to the sound of a person breathing tranquilly in sleep. Feeble as is this comparison, a better has not been suggested. The comparison by Skoda to the sound of air sucked in by the lips, is quite as indefinite. It has been compared to the sound of the wind passing through foliage. This is not an improvement on the comparison by Laennec, except that it has something of a poetic savor. From this comparison the quality of the murmur has sometimes been called breezy. Analyzed with reference to its component characters, the intensity varies so much in different healthy persons that nothing distinctive is to be derived from this source. The pitch, as compared with that of the more important of the morbid respiratory sounds, is low. Here is one distinctive feature. The quality, like that of the normal resonance on percussion, is *sui generis*. It cannot be described by words which give any definite idea of it. It has no close analogy to any other sound produced outside of the body. It can only be correctly appreciated by direct observation. The quality may be called vesicular for the same reason that the quality of the normal resonance on percussion is so called; in each of these normal signs the quality is incident to the vesicular structure of the lungs. This structure is so peculiar that it cannot be fully represented by any artificial contrivance by which the movements of air within the structure may be imitated; hence, the fact that the quality of the respiratory murmur, as well as that of the normal resonance on percussion, is *sui generis*. These characters of pitch and quality relate to the inspiratory sound. The relative characters of the expiratory sound are of importance in distinguishing the normal murmur from certain morbid respiratory signs. The expiratory sound is much shorter than the inspiratory, and also much weaker. It has no vesicular quality. I know of no better way of expressing the quality than by calling it simply blowing. It is not unlike the sound of the expired breath from the open mouth.

Laennec's explanation of the vesicular respiration was that it is due to the friction of the air in the pulmonary alveoli. I need not take time to show that this explanation is unsatisfactory. To say, as does Skoda, that the murmur is due to the resistance which the cells offer to the air, is simply to state a fact without any explanation. Other explanations, alike un-

satisfactory, are, that it is due to contraction of the bronchial muscular fibres in the act of inspiration, as held by Blakiston and Leaning; and, as held by Zamminer and Seitz, that it is produced at the mouths of the infundibula, in the same way as sounds by blowing over the opening of a hollow key. Eichhorst, author of a German treatise on *The Methods of Physical Investigation in Internal Diseases*, (1881), after citing these explanations, assumes that it is a physical impossibility for the respiratory sounds to be produced by the currents of air within the pulmonary air passages, and he considers, therefore, that their source must be within the larynx, being there produced by the projection of the vocal cords, and conducted downward by the air tubes. This is a revival of the doctrine of M. Beau, author of a work on *Auscultation* (1856); an author whose fantastic theories were deemed of sufficient consequence to be always quoted by contemporaneous writers, but almost invariably quoted in order to dissent from them. This glottic theory of the mechanism of the vesicular murmur of respiration is readily disposed of by a very simple experiment. Remove the lungs from the body and separate them from the trachea. Now, if respiration be imitated by means of the bellows, the nozzle being introduced into a primary bronchus, or by means of one of the flexible tubes connected with Davidson's syringe, and the lung be auscultated, either by immediate auscultation or the stethoscope, the vesicular murmur is reproduced, much intensified as compared with the murmur in life. This experiment, assuredly, disproves the physical impossibility of the production of respiratory sounds within the pulmonary air passages, and, consequently, the glottic theory of the production of the vesicular murmur.

For the true explanation (as it seems to me) of the vesicular murmur I am unable to give credit to any writer. In my work on *Physical Exploration*, published in 1856, I submitted the following inquiry: "May not the peculiar quality be owing to the separation of the sides of the cells and the capillary tubes which, to a greater or less extent, come into contact, and, owing to the moisture of the tissues, are slightly adherent during the collapse of the lung incident to expiration?" And I added, "We shall see hereafter that this is the most rational explanation of an important and highly distinctive physical sign of disease." Allusion in the latter sentence was made to the crepitant râle. It is noteworthy that Laennec, in describing the differences between the vesicular murmur and bronchial respiration, says that the latter loses the "slight crepitation" which belongs to the former. The expression "slight crepitation" distinguishes more accurately than any other term the peculiar quality of the vesicular murmur, and, at the same time, it denotes the mechanism. The explanation which, more than a quarter of a century ago, I submitted in the form of an inquiry, I have ever since taught as the true explanation. As corroborative of its truth, I submit the following experiment:

Take the lungs of any animal of sufficient size (a sheep or a calf), twelve or twenty-four hours after the animal has been killed, and introduce the nozzle of a pair of bellows into either the trachea or one of the primary bronchi. Imitate respiratory acts by means of the bellows. If the stethoscope be placed directly upon the lungs, or with a folded napkin intervening, with each inflation a crepitating character of the sound is apparent. The inflation of portions which contained but little or no air, *i. e.*, collapsed portions, gives a perfect representation of the crepitant râle. Now if the intervening folds of cloth between the stethoscope and the chest be sufficiently increased in thickness, the crepitating character is modified, and the quality becomes hyper-vesicular; in other words, we obtain an intensified representation of the vesicular respiration. The lungs from the human body will, of course, answer as well as those of the calf or sheep. The crepitation may disappear after the inflations by means of the bellows have been continued for some time. The expression by Laennec, "slight crepitation," therefore, expresses not only the character of the murmur, but the mechanism of its production.

I refer to an experiment made by Penzoldt, and cited by Eichhorst, in his work[1] on the methods of physical exploration, as exemplifying either the disadvantage of not having studied respiratory sounds analytically with reference to pitch and quality, or the influence of the imagination on the observation of sounds, or, perhaps, of both these two sources of error. It is stated that if a portion of a solid organ, the liver, for example, be placed over the larynx of a healthy subject, the laryngeal respiration is transmitted to the ear of the auscultator without change; but if a portion of inflated lung be so placed, the laryngeal respiration becomes changed, by its transmission, into a vesicular respiration. This latter statement is adduced by that author to show that the vesicular murmur of respiration is not produced within the pulmonary alveoli, but that it is the laryngeal respiration conducted through air vesicles. Let any observer, not biased by theoretical expectations, and familiar with the distinctive characters of the vesicular and the laryngeal respiration as determined by analytical study, repeat this simple experiment, and it will be found that there is no essential difference between the sounds as transmitted through the solid organ and the inflated lung. The inspiratory sound does not lose its tubularity, acquiring in its place the vesicular quality; and the relative pitch of the inspiratory as compared with the expiratory sound, is the same as when the stethoscope is placed immediately upon the integument covering the larynx. I make this statement after having repeatedly made the experiment. Whether this statement or that of Penzoldt is correct, can be readily determined by any one who will take the little trouble requisite for repeating the experiment, and comparing the sounds analytically and impartially.

BRONCHIAL RESPIRATION.

Of the morbid respiratory signs, the one which offers, in its distinctive characters, the strongest contrast to those of the normal vesicular murmur, is the *bronchial respiration*, and there is an advantage, therefore, in considering first this sign. Let me enumerate the distinctive characters of the bronchial respiration, as determined by analytical study, although, doubtless, they are familiar to those whom I now address. The intensity of both the inspiratory and the expiratory sound is often greater than that of the vesicular respiration; but this is not an essential feature. The inspiratory sound has no vesicular quality, but, in place thereof, a quality expressed by the term tubular. It is identical with the sound of a current of air through a tube. The pitch is high. The expiratory sound, usually more intense than the inspiratory, has a tubular instead of the simple blowing quality of the expiratory sound in the normal vesicular respiration; it is higher in pitch than the inspiratory sound; it is prolonged to the length of, or more, of the inspiratory sound, and, instead of being continuous with the latter, as is the case with the inspiratory and the expiratory sound in the normal vesicular respiration, the two sounds are separated by a brief interval of time. The latter char-

[1] Lehrbuch der Physikalischen Untersuchungsmethoden innerer Krankheiten, von Dr. Hermann Eichhorst, Braunschweig, 1881.

acter is due to the fact that the inspiratory sound ceases a little before the cessation of the inspiratory act.

No one doubts that the sign having the characters just enumerated is heard over solidified lung. It occurs, therefore, in the second stage of pneumonia, over lung compressed into a solid mass by the pressure of liquid or air within the pleural space; also, in cases of phthisical exudation and induration, as well as in other affections which solidify the pulmonary structure. That the sign represents exclusively solidification of lung, however, is not universally acknowledged. The opinion is held by many that it represents, also, pulmonary cavities. When I come to consider the cavernous respiration, I shall undertake to demonstrate that this opinion is erroneous. I believe the bronchial respiration to be the respiratory sign of complete or considerable solidification of lung, and of no other morbid physical condition. The only room for doubt as regards this limitation of the significance of the sign, is afforded by cases of dilatation of the intra-pulmonary bronchi. In these cases, as it seems to me, the presence of this sign is due, not to the bronchial dilatation, but to the condensation of lung surrounding the dilated tubes. The artificial illustration of this sign is easy with Davidson's syringe. If the tube conducting from the central bulb be placed close to the ear, and covered with the hand, in order to exclude extraneous noises, the current of air produced by compression of the bulb causes a well-marked tubular sound. A similar sound is heard when the tube conducting to the bulb is placed close to the ear, the sound in this instance being caused by an expansion of the bulb. If we attach to the tube conducting from the central bulb other tubes of more or less length, and varying in size, the tubular sound is heard at any point with an equal force of the current of air, and the pitch is found to be somewhat higher the smaller the size of the tube. Blowing into the tube with the mouth will answer as well as Davidson's syringe; and it is not necessary that the current be strong in order to produce a tubular sound. I oppose this simple experiment to the statement by Eichhorst that it is a physical impossibility for the movement of air in the bronchial tubes to produce a sound. It seems surprising that an author should make this statement when it may be disproved by an experiment which can be made at any moment.

The mechanism of the bronchial respiration, of course, involves the passage of air in tubes; but it is a question in what tubes is the sound produced when this sign is heard over the chest. Laennec's explanation was simple, and, in the main, it has not been disproved. He referred the sound chiefly to the passage of a current of air in the larynx, trachea, and the large bronchi at the root of the lungs. He accounted for the absence of the sign over the healthy chest, first, by the presence of the vesicular murmur, which drowns the bronchial sound; and, second, by supposing that the air vesicles containing air conduct sounds less readily than solidified lung. The better conduction of sounds by solidified lung than by the normal inflated lung was denied by Skoda. Skoda based his denial on physical principles and on certain experiments relating to the conduction of the voice. I shall refer to these experiments in connection with vocal signs. I will simply say here that I have repeated them, and with a result the same as stated by Skoda. Another experiment by Skoda is easily repeated; namely, placing successively over the larynx portions of solidified lung and of inflated lung, equal in volume, and comparing the transmission, through each of these media, of the laryngeal respiration. This experiment shows, according to my observations, that Laennec was in error in supposing that solidified lung is a better conductor of sound than healthy lung. It has been asserted that if a watch be placed alternately beneath portions of solidified and of healthy lung, of equal volume, auscultation shows the solidified lung to be the better conductor of sound. This experiment I have repeatedly made, and with a result the reverse of the assertion that the solidified lung is a better conductor. I may add that up to a recent date I had believed, in accordance with the general belief at the present time, that sounds were better conducted by solidified than by healthy lung containing air.

Laennec supposed that a current of air within the intra-pulmonary bronchi, and even in those of small size, coöperated in producing the bronchial respiration. This supposition has been deemed improbable, at least when an entire lobe is solidified, as in cases of lobar pneumonia. Skoda and others have maintained that a current of air cannot take place within a solidified lobe. This view seems not irrational, considering that the lobe is enlarged to the limit of a full inspiration, that its volume diminishes very little with expiration, and enlarges as little with inspiration, and that the respiratory movements of the chest on the affected side are more or less restricted. Experiment, however, shows that air passes freely through the bronchial tubes within a lobe solidified by pneumonia. In a lung removed from the body, the upper lobe completely solidified and the upper two-thirds of the lower lobe solidified, the unsolidified portion of the lower lobe was readily inflated either by the bellows, or by the breath, the current being inserted either into the trachea or the primary bronchus. Moreover, in the condition just stated, a vesicular respiratory murmur was appreciable over the lower third of the lower lobe during life, as well as by inflation after death, a fact showing the free passage of a current of air within the intra-pulmonary bronchi of the solidified upper two-thirds of the lobe. I assume, therefore, that it is incorrect to say that air does not pass into the bronchial tubes within a lobe which is completely solidified. Assuming this, the question then is, what part does the air in the intra-pulmonary bronchi have in the production of the bronchial respiration? That Laennec was right in supposing a tubular sound to be produced by a current of air in these tubes is not irrational. Such a sound, in fact, may be produced after death by a current of air from the bellows or the mouth directed into the bronchus connected with a solidified lobe.

But there are grounds for attributing the bronchial respiration, chiefly, if not exclusively, to the larynx and trachea. The fact that in essential characters relating to pitch and quality, the normal laryngeal and the tracheal respiration are identical with those of the bronchial respiration, is perhaps sufficient in itself to prove that the latter is in reality the former conducted into the solidified lung. How can this be, if solidified lung be not a good conductor of sound? The answer to this question is, the conduction is not by the solidified lung, but by air within the intra-pulmonary bronchi. This, as it seems to me, enters into the explanation of the bronchial respiration. The air in the intra-pulmonary bronchi is the conducting medium, as it is in the stethoscope. The explanation does not conflict with the fact that solidified lung is a poorer conductor of sound than healthy lung. For good conduction of sound it is not necessary that the conducting column of air be large. The space containing air within the tubes of the binaural stethoscope, is not larger than that in the medium sized bronchi. The explanation is consistent with the absence of bronchial respiration when the bronchial tubes are obstructed by either an accumulation of morbid products, or by pressure from without. The following experiment illustrates the conduction by the air within the intra-pulmo-

nary bronchi: In a lung from a body dead with acute pneumonia, the upper lobe was completely solidified. When a current of air from the mouth was directed into the trachea, and the stethoscope applied to the solidified lobe, a well-marked bronchial respiration was appreciable. By compressing with the fingers the bronchus leading to the solidified lobe, a very feeble and distinct respiratory sound only was perceived. A well-marked bronchial respiration returned when the compression was suspended. Dr. Powell, of London, cites an experiment made by MM. Boudet and Chaveau, as demonstrating the conduction from the larynx and trachea of the bronchial respiration. On a horse affected with pneumonia, well-marked bronchial respiration over the solidified lung was ascertained. Tracheotomy was then performed, and when the wound in the trachea was held widely open, the bronchial respiration disappeared, while exaggerated vesicular respiration over the other lung continued. On introducing a tube within the wound the bronchial respiration over the solidified lung returned. This experiment illustrates not only the fact of the conduction from the larynx and the trachea, but also that the conducting medium is the air within the bronchial tubes.

Experimental observations thus appear to prove conclusively that suppression of the vesicular murmur and conduction of the tracheal and laryngeal respiration are the two factors in the mechanism of bronchial respiration, admitting that solidification does not render the lung a better conductor of sound. This admission, however, will be found to be not easily accorded with certain clinical facts. To some of these reference will be made in connection with vocal signs. One difficulty relates to bronchial respiration. If lung containing air within the alveoli be a better conductor of sound than solidified lung, why is it that we do not have bronchial respiration when the vesicular murmur is suppressed by emphysema? Here is an evident inconsistency. If, however, the explanation which has been given of the bronchial respiration be proven, it is not disproved by apparently conflicting facts. The proper course to pursue is to seek to reconcile these with the explanation. I am not prepared to say how this is to be done in the instance just cited.

The higher pitch of the expiratory, as compared with the inspiratory, sound in the bronchial respiration accords with what is observed when the stethoscope is placed upon the larynx or trachea. The explanation is the narrowed orifice at the glottis by an approximation of the vocal cords in the act of expiration, when compared with the separation of the cords which takes place in the inspiratory act.

CAVERNOUS RESPIRATION.

From the bronchial I pass to the *cavernous respiration.* Laennec recognized the existence of a cavernous respiration, but he regards it as having the same characters as the bronchial respiration, its distinctive feature being a perception as if the air entered a space larger than that of the bronchial tubes. He described two modifications, in one the air seeming to enter and emerge from the ear of the auscultator, and in the other the sound giving the idea of a movable veil between the cavity and the ear. He called the former of these modifications, a blowing respiration, and the latter veiled blowing. It must be admitted that Laennec's account of cavernous respiration is indefinite and unsatisfactory. The criticisms of Skoda are undoubtedly just. But Skoda fell into an error greater than that of Laennec, with regard to this sign, for he denied *in toto* the existence of a cavernous, as distinct from bronchial respiration. He says (quoting his language), "I consider Laennec's bronchial and cavernous respiration to be one and the same murmur; his blowing bronchial to be a loud bronchial murmur, and his *souffle voilé* to be an unimportant modification of the bronchial respiration." So great has been the influence of Skoda's teachings, that the most recent German writers hold to the identity of the characters of the bronchial and the cavernous respiration. The individuality of cavernous respiration has been, and is acknowledged by English and French authors, but its differential characters were not distinctly indicated prior to 1852. I believe that I do not assume too much in saying that these characters were first fully pointed out in the prize essay published in that year, to which I have already referred, and in my work on *Physical Exploration*, published in 1856. My description of cavernous respiration was based on the analytical study of respiratory sounds with reference to pitch and quality, conjoined with autopsical examinations. In the instances given in my essay, the examinations after death were made, and written reports furnished, by Prof. John C. Dalton.

With reference to its distinctive characters, the cavernous respiration is to be contrasted, on the one hand, with the bronchial respiration, and, on the other hand, with the normal vesicular murmur. Contrasted with the bronchial respiration, the points of difference are not less marked than those which distinguish the bronchial respiration and the normal vesicular murmur. The cavernous inspiratory sound has no tubular quality, and is low in pitch; the expiratory sound is usually more feeble than the inspiratory, its duration or length variable, and its pitch is lower. With an appreciation of these differential characters, the cavernous and the bronchial respiration cannot possibly be confounded. The quality of the sound in both the cavernous inspiratory and expiratory sound may be called blowing, in distinction from a tubular quality. The quality is like that of the expiratory sound in the normal vesicular murmur. The contrast of the cavernous respiration with the normal vesicular murmur is less strong than with bronchial respiration. In both the cavernous respiration and the normal vesicular murmur, the pitch of the inspiratory and of the expiratory sound is low, and the expiratory is lower in pitch than the inspiratory sound. The essential point of difference relates to quality. The vesicular quality is wanting in the cavernous respiration. Given a respiratory sign in which the inspiration is non-vesicular and non-tubular, with lowness of pitch, the expiration having the same quality but still lower in pitch, the sign can be no other than cavernous respiration. In confirmation of the presence of this sign, clinically, circumstances other than its distinctive characters may be taken into account. It is limited to a circumscribed space; around this space the respiratory sound is either vesicular or the signs of more or less consolidation are present, the latter being true in a large proportion of instances; the coexisting vocal signs and those obtained by percussion are indicative of cavity, and evidence is sometimes obtained by inspection.

Artificially the cavernous respiration may be illustrated by the following simple experiment: The cavity is represented by an India-rubber balloon of the size of a large orange, with thin walls, and two openings connected with a tube of greater or less length. Attaching to one of the tubes a pair of bellows, or, what answers equally well, using the breath from the mouth, the balloon is inflated and the air withdrawn in imitation of the respiratory acts. Placing a binaural stethoscope over the balloon, or listening with it close to the ear, the movement of the air into it and out of it gives rise to a low-pitched blowing sound, the outward lower in pitch than the inward current. These observations, identical with those of the cavernous respiration, may

be contrasted with tubular breathing produced artificially in the manner already stated.[1]

The sign may also be reproduced within a cavity with flaccid walls in a lung removed from the body. I had recently under observation a hospital case in which there was well-marked cavernous respiration at the summit of the chest. After death, in that situation was found a cavity of the size of a large orange, the anterior wall of which consisted of only thickened pleura, and collapsed when the lung was removed from the body. The cavity was readily and largely inflated by the breath directed into the trachea. With the stethoscope applied upon the lung, over the cavity, a loud, low-pitched, blowing sound was perceived when the air entered and when it escaped from the cavity. It is not difficult for those connected with large hospitals to obtain this demonstration of the distinctive characters of the cavernous respiration.

AMPHORIC RESPIRATION.

Amphoric respiration is to be regarded as a variety of the cavernous, and claims but a few words. As is well known, it is a sign *par excellence* of perforation of lung and pneumothorax; but it is not a very infrequent sign of a pulmonary cavity. Whenever present, if pneumothorax be excluded, it is diagnostic proof of a cavity with rigid walls, that is, walls which do not expand notably with inspiration, and collapse with expiration. As produced in cases of pneumothorax, it may be represented artificially by blowing through a small tube into an inflated India-rubber bag of considerable size. As produced in a pulmonary cavity, it is represented by directing a current of air from the mouth or a pair of bellows over the opening into an India-rubber ball of the size of an egg or an orange. It may be demonstrated to be a sign of a cavity with rigid walls in a lung removed from the body. In the specimen just referred to, of a cavity with flaccid walls, which furnished a cavernous respiration after death and during life, a little below this cavity the lung was solidified; but within a circumscribed space well-marked amphoric respiration was perceived when a current of air from the mouth was directed into the lung. An incision revealed a cavity of about the size of an English walnut, surrounded on all sides by solidified lung.

BRONCHO-VESICULAR RESPIRATION.

The three signs which have been considered, namely, the normal vesicular murmur, the bronchial respiration, and the cavernous respiration (the first of these, a normal sign, and the two others abnormal signs), may be said to constitute the simple types of the respiratory sounds heard over the chest. Other signs consist of the characters of these in combination, and may, therefore, be distinguished as compound types. Of these signs, the one which is of most importance I shall consider under the name *broncho-vesicular respiration*. This name was proposed in my work on *Physical Exploration*, published in 1856. It has been adopted, to some extent, by writers in this country. Prof. Da Costa prefers the term *vesiculo-bronchial*. The pressing need of a term expressive of certain morbid auscultatory sounds must, as it seems to me, be evident to any one who has given attention to the study of these sounds. The morbid physical conditions represented by the sounds referred to are the varying degrees of solidification of lung, falling short of a degree sufficient to give rise to a purely bronchial respiration. In a purely bronchial respiration, the inspiratory sound is devoid of any vesicular quality; the quality is entirely tubular. In the broncho-vesicular respiration, the inspiratory sound is both tubular and vesicular; that is, it consists of these two qualities combined. The tubular and the vesicular quality may be combined in different proportions; in some instances the vesicular, and in other instances the tubular, quality predominates. The predominance of the one or of the other of these qualities depends on the degree of solidification; if the solidification be but slight, the vesicular quality exceeds the tubular, and, *per contra*, if the solidification be nearly sufficient for the purely bronchial respiration, there is but little of the vesicular quality, the tubular being in excess. The broncho-vesicular respiration, thus, as a representative sign, covers all the modifications of respiratory sounds, denoting solidification, between the normal vesicular murmur and a purely bronchial respiration. And by means of this sign it is practicable, not only to recognize the existence of solidification which is insufficient in degree to give rise to the bronchial respiration, but to judge of the degree of solidification, that is, whether it be very slight, slight, moderate, or closely approximating to that requisite for a purely bronchial respiration. The combination of the vesicular and the tubular qualities carries with it other characters which correspond to the different proportions in which the two qualities are combined. In proportion as the vesicular quality predominates in the sound of inspiration, the pitch of the sound is low; and, conversely, the pitch is raised in proportion as the tubular quality predominates. The expiratory sound is prolonged, high in pitch, and tubular in quality, in proportion as the inspiratory sound is high in pitch and tubular in quality; in other words, in proportion to the degree of solidification; and, conversely, the expiratory sound is less prolonged, less high, and less tubular in proportion as the vesicular quality in the inspiratory sound predominates; in other words, in proportion as the degree of solidification is small.

The name broncho-vesicular is intended to supersede such terms as rude, rough, and harsh respiration. These terms are not only inappropriate, but they lead to error. An exaggerated vesicular respiration without solidification of lung may be ruder or more harsh than the sound which represents the latter condition. An imperfectly developed dry bronchial râle may give roughness to the respiratory murmur. These terms, thus, do not denote any fixed, definite, morbid physical condition, and erroneous inferences are liable to be drawn from them in cases of disease. Still more unsatisfactory is the name *indeterminate respiratory murmurs* introduced by Skoda, which embraces the abnormal sounds expressed by the term broncho-vesicular. Under the name indeterminate Skoda includes, quoting his language, "Respiratory murmurs having neither the character of vesicular nor of bronchial respiration." He admits that "No distinct indication can in any particular case be drawn from such a murmur," and he adds, "All respiratory murmurs which give us no information as to the state of the parenchyma of the lungs I call indeterminate respiratory murmurs, and any subdivision of them appears to me to be useless." The name "indeterminate" with the meaning as defined by Skoda, is still in vogue with German writers. The name is applied to sounds due to simple bronchitis, as well as to phthisis and other affections. It must be sufficiently obvious that with the confused idea of the sign, a confusion implied in the name "indeterminate," it cannot be of much practical value in diagnosis. It is far otherwise with the broncho-vesicular respiration, its characters having been determined by analytical study. The sign is of great practical value in the diagnosis of phthisis, in

[1] The sign is not produced by blowing into a balloon with but one opening. I infer, therefore, that for the cavernous respiration in life, cavities must have openings for the exit as well as the ingress of air.

the early stage of pneumonia, and the stage of resolution, and in other pulmonary affections involving partial solidification of lung. The sign represents the latter condition, and nothing else. It cannot be produced by a simple bronchial affection. It is not less determinate as regards its distinctive characters and its pathological significance, than any of the respiratory signs.

A case of pneumonia during the stage of resolution affords illustrations of all the gradations of this sign. The sign is present as soon as absorption has removed the contents of a sufficient number of air vesicles for a vesicular quality to be perceived in the sound of inspiration. The tubular quality now predominates, and the respiratory sound is still prolonged, high, and tubular. With each successive day, as absorption progresses, the vesicular quality in the inspiratory sound increases, and the tubular quality diminishes. With these changes in the inspiratory sound, the expiratory sound on each successive day is less prolonged, less intense, less light in pitch, and less tubular in quality. At length, resolution being complete, the vesicular quality becomes, for a time, more marked than in health, all the characters of the broncho-vesicular respiration having disappeared.

The characters of the broncho-vesicular respiration may be studied as illustrated in the healthy chest. In the infra-clavicular region, especially the sternal portion, and in the interscapular region, the respiratory murmur, as compared with that over other parts of the chest, is broncho-vesicular; that is, owing to the proximity of the larger bronchi, the characters of the bronchial and of the vesicular respiration are combined. This is more apparent on the right than on the left side. The respiration in these situations has been called the *normal bronchial respiration*. This expression is inexact, inasmuch as the respiratory sounds are not purely bronchial. It is more correct to say that in these situations there is a *normal broncho-vesicular respiration*. This normal broncho-vesicular respiration may be still better observed by auscultation of the lungs removed from the chest. With the human lungs or those of the calf or sheep, if the nozzle of a pair of bellows be introduced into the trachea, the respiratory acts imitated, and the stethoscope placed either on the lung, or a thin layer of cloth only interposed, the mixture of the characters of the bronchial and the vesicular respiration at and near the apex of the lungs, will be rendered very marked by contrast with the respiratory sounds over other portions.

BRONCHO-CAVERNOUS RESPIRATION.

The combination of the characters of the bronchial with those of the cavernous respiration, and of the cavernous with the vesicular murmur, constitute other compound types. Seitz, the editor of the later editions of Niemeyer's work on the *Practice of Medicine*, has described a respiratory sign which he calls a "*metamorphosizing respiratory sound*." The metamorphosis is in the inspiratory sound. The sound at its beginning is described as rude, and the last two-thirds as bronchial. Bearing in mind that with German writers there is no cavernous, as distinguished from bronchial, respiration, I infer the latter part of the respiratory sound to be cavernous. Such a metamorphosis was described by me in my prize essay, published in 1852, together with the physical conditions found after death. This is one variety of a broncho-cavernous respiration. The first part of the inspiratory sound is a bronchial respiration. It takes place before the air has entered freely into a cavity. One of the characters of the cavernous inspiration is that it is evolved slowly. When the air enters freely into the cavity, the bronchial is superseded by the cavernous respiration. The sign thus denotes a cavity, with proximate solidification of lung. Another variety is an association of a bronchial expiration with a cavernous inspiration. This variety is not infrequently met with. The explanation is simple. In bronchial respiration, the expiratory sound, as a rule, is more intense than the inspiratory. The inspiratory sound may be relatively quite feeble, and it is sometimes wanting. On the other hand, the expiratory sound in cavernous respiration, as a rule, is feeble, and may be quite so, or it may be wanting. Now, it is easy to understand that when a cavity is situated near a portion of solidified lung, the auscultator may obtain over the cavity an inspiratory sound which is cavernous, that is, low in pitch and blowing in quality, associated with an expiratory sound which is bronchial, that is, high in pitch and tubular in quality.

VESICULO-CAVERNOUS RESPIRATION.

The characters of the vesicular and of the bronchial respiration are combined when the pulmonary structure surrounding a cavity remains intact, or but little affected. The vesicular quality is then derived from the surrounding lung. The recognition of this compound type may seem a refinement in auscultation, but that in certain cases it is readily recognized, clinically, I am well satisfied. It may be called a *vesiculo-cavernous respiration*.

DIMINISHED AND SUPPRESSED VESICULAR MURMUR, AND INTERRUPTED RESPIRATION.

Diminished vesicular murmur, without change in pitch and quality, and suppression of the murmur, which are physical signs of much value, taken in connection with other signs and with symptoms, do not here claim consideration. Of the sign called interrupted, wavy, and cog-wheeled respiration, I will only remark that, as an isolated sign, it has but little clinical importance; it derives whatever value belongs to it from its association with other signs. I shall conclude this discourse with some remarks on prolonged expiratory sound, either existing without an inspiratory sound, or when the latter is too weak in its character to be distinctly appreciable.

PROLONGED EXPIRATION.

It is remarkable that Laennec should have given so little attention to the study of the sound of expiration. James Jackson, the younger, was the first to appreciate the clinical value of a prolonged expiratory sound. As early as 1832 he called attention to the significance of a prolonged expiratory sound at the summit of the chest in the diagnosis of pulmonary phthisis. He noticed not only the prolongation, but its resemblance in character to the prolonged expiration in bronchial respiration. Since his observations this sign has been included in the group of signs to be sought after in the diagnosis of phthisis. It may or may not have significance in relation to that disease. A prolonged expiration which may be more or less high in pitch and tubular in quality, is not very infrequently observed in healthy subjects at the summit of the chest on the right side. It belongs to the normal broncho-vesicular respiration, and may be present when the characters of this sign, owing to feebleness of the inspiratory sound, are not appreciable. I have known a prolonged high-pitched expiration to exist on both sides at the summit of the chest, no other morbid signs being therewith associated. Absence of other signs of disease is the fact to be relied upon in judging that the prolonged expiration is a normal peculiarity. But, as a morbid sign, prolongation of the expiratory sound is not always evidence of phthisis nor of any affection involving solidification of lung. It is a sign in cases of pulmonary emphysema, and may occur whenever the expiratory act is increased in force and length, or whenever

there is an obstacle to the free exit of air from the smaller to the larger bronchial tubes. How is this difference in the significance of the sign to be recognized clinically? This question can be definitely answered. If the prolonged expiratory sound be high in pitch and tubular in quality, exclusive of the instances in which it has these characters as a normal peculiarity at the summit of the chest on the right side, the sign denotes lung solidified by a phthisical or some other solidifying morbid process. It has precisely the same significance as when it is associated with a high-pitched tubular inspiratory sound in the bronchial respiration. If, on the other hand, the prolonged expiratory sound have characters, as regards pitch and quality, the same as in the normal vesicular murmur, differing only in length and intensity, it is not a sign of phthisis, nor of any other affection involving solidification of lung. This variety of prolonged expiratory sound, associated with other signs, is diagnostic of pulmonary emphysema.

These differential characters pertaining to the sign, the difference in its significance being correspondingly marked, exemplify the importance of the analytical study of auscultatory phenomena. With few exceptions in works treating of auscultation at the present time, a prolonged expiratory sound, as a morbid sign, is considered without reference to the differences in pitch and quality on which depend its diagnostic significance. If these differences be not taken into account, the sign is as likely to lead to error as to a correct diagnosis.

ORIGINAL ARTICLES.

AMPUTATIONS, AND THEIR ANTISEPTIC DRESSINGS.

By J. HENRY C. SIMES, M.D.,
ASSISTANT SURGEON TO THE EPISCOPAL HOSPITAL, PHILADELPHIA.

ANY experience in any method of treatment, whether the result proves favorable or unfavorable, is, if carefully noted, a means of assisting the treatment in similar cases which may occur at a future period. More especially is this a fact when applied to those methods which from their novelty or theoretical claims are urged by their discoverers.

Antiseptic surgery, at the present time, can scarcely be considered as a new method of treatment in surgical operations, nor are its claims now based entirely upon theoretical grounds; yet it is far from being accepted as a general method of treatment; more particularly is this true of American surgeons. In this city I know of no hospital in which its surgeons have fully and thoroughly carried out the details necessary to give their treatment the name of antiseptic; that is to say, they have not attended to the minute directions, and in many cases the principal features have been omitted in their operations. For these reasons I have thought a brief outline of my experience, during the past summer while on duty at the Episcopal Hospital, would perhaps aid in assisting to form some conclusions as to the value of antiseptic dressings when applied to amputations.

My reasons for not continuing the usual mode of treatment, viz., non-antiseptic, and adopting the antiseptic, was that upon beginning my term of service it was found that there had been for the previous spring and winter months, an almost uninterrupted series of secondary complications (erysipelas, cellulitis, etc.) following the amputations which had been performed in the hospital; these complications were not limited to this hospital alone, but upon further investigation it was ascertained that in most of the hospitals throughout the city, similar sequelæ to operations had prevailed. Therefore, it seemed to me a change of some kind was justifiable, in order to endeavor to check the unfavorable course taken by the process of healing after amputations. The antiseptic dressing had not been, during this year, employed in the hospital. The opportunity to test its usefulness or not, as the case might be, here presented itself, and a trial was determined upon.

The procedure followed, in the cases coming under my care, was essentially that recommended by Lister, and consisted, in outline, of the carbolized spray; catgut ligatures; close apposition of the wound by means of silver sutures; thorough drainage, obtained by the use of rubber drainage-tubes of large calibre (one-fourth of an inch); washing the wound with carbolized water; oil-silk protective over the line of incision; carbolized gauze wet with carbolized water; dry carbolized gauze; rubber cloth, which was securely kept in close relation to the parts by the employment of a narrow elastic bandage at its margin; and, finally, the whole held in position by a muslin roller. The instruments used during the operation were placed in a tray of carbolized water.

The results obtained by the employment of the above dressing were very satisfactory, as may be seen from the following cases. These cases, seven in number, are not given in full, but they may be summarized by stating that they consisted of two amputations of the forearm, two amputations of the arm, one amputation at the knee-joint, one resection of the knee, and one amputation of the female breast. In none of the cases was there any secondary complication, no erysipelas, no cellulitis, no abscess. Certainly here was a change for the better, since only a month previous one or all these complications accompanied similar operations, and the questions which naturally presented themselves were: Was the favorable change owing to a change in the manner of dressing? Was it due to a change in the time of the year? or, Was it dependent upon any other outside influence?

These were all very satisfactorily answered to my mind, and convinced me that there was but one cause to account for the results which so very markedly differed from those previously obtained. The, to me, convincing circumstance exists in the progress of other operations performed in the same hospital, at the same time and treated in the same wards, but under a different method of treatment; the dressing was not antiseptic, and the results were not so favorable. These cases were one amputation of the forearm, three amputations of fingers, one amputation of thumb, and one amputation of toe. In all of the above cases there occurred either a complication of erysipelas, cellulitis, abscesses, or all three. When contrasted with the former cases—those dressed antiseptically, there are observed very evi-

dent and important differences. Here are two series of cases, both placed under the same surrounding influences, both receiving the same care and attention, yet each following a different course. A cause for such a distinct and marked difference in the course of the two series is to be expected and naturally sought after, and the only appreciable one is found in the different methods employed in dressing the cases. In nothing was there any known variation made in treating the two series, except in that of the different dressings—the one antiseptic, the other not; and it would seem only reasonable to conclude that this difference was the one which influenced the ultimate results.

Not only were these differences of secondary complications following the operations noticeable, but there was also a great variation in the symptoms which followed as a result of operative interference; and when comparing the two series one with another, it was found that the cases dressed antiseptically presented a much more favorable record than those cases in which this method of treatment had not been employed.

A symptom common to both series, but one in which the difference was most decidedly marked, was that of *pain*. In the cases in which the non-antiseptic dressing was used this symptom was always present, and habitually demanded anodynes for its relief; while, on the contrary, with the antiseptically dressed cases, the pain was so slight—indeed it may be said absent—that in none were anodynes indicated, and their administration was omitted during treatment.

The amount of *traumatic fever*, in the two series of cases, differed in a very appreciable degree. In the cases in which the antiseptic dressing was used, this symptom was generally very poorly defined, and its presence only indicated by the employment of the thermometer; while in the non-antiseptic dressed cases, there was no exception: each case had a distinct and quite evident attack of surgical fever. This variation in the relative amount of fever was very apparent when the temperature records taken during the progress of healing of the wounds were contrasted. Thus, the average maximum temperature in the antiseptic dressed cases was 99.6° Fahr., while in the non-antiseptic it was 102.5° Fahr.

The *aspect* of the wounds in the two series of cases was very different. The cases in which the antiseptic dressing was used presented a perfectly normal appearance of the surrounding parts; there was an entire absence of all redness and tumefaction, only a very faint red blush was observed along the line of incision and surrounding the punctures produced by the wire sutures. In the cases not treated antiseptically, there were seen, on the contrary, a marked diffused redness and considerable œdema of the neighboring tissues.

The symptom of *tenderness* upon pressure, in the two category of cases, was singularly unlike. In the one—the antiseptic dressed, it was so slight as scarcely to merit a place as a symptom; while in the other it was a very prominent and always present feature.

The *length of time* necessary to complete the healing of the wounds in the two series of cases varied very much. Those cases in which the antiseptic dressing was used healed much sooner than those in which it was not. This circumstance was, undoubtedly due to the secondary complications which interfered with the process of healing in the latter cases.

There is one point more I wish to speak of, and it is in reference to the application of the antiseptic dressing. In reading over the directions given by the advocates of this method of treatment, it does appear a very formidable and extremely tedious one. But in its practical application, however, I am quite satisfied, from my experience with it, that these seeming drawbacks to its use do not exist. It is something out of the usual routine, and the first trials made with it may, and indeed do, call for a little more time and attention on the part of the surgeon; yet with a little experience, and the assistants and nurses becoming familiar with its application, the dressing can be applied with as great facility as that ordinarily employed. A circumstance not to be overlooked, and it also bears upon the question of time occupied in applying this method of treatment, is that the renewal of this dressing is not required as often as the non-antiseptic. Most of the cases which were dressed antiseptically had the dressing renewed two or three times in the course of a week, frequently the interval was longer—once in four or five days. In the non-antiseptic dressed cases, it was generally necessary to renew the dressings daily, and they never remained longer than two days without renewal. Therefore, it is seen that the time actually occupied in dressing these two series of cases was much longer in the non-antiseptic than in the antiseptic.

TOTAL LOSS, BY SLOUGHING, OF BOTH PAROTID GLANDS.

BY C. M. RAMSDELL, A.M., M.D.,
OF LAMPASAS, TEXAS.

ON the 26th of August, 1882, my brother, Dr. F. R. Ramsdell, was called into the country to see a case of fever. The patient was Mr. Lewis C., white, carpenter, æt. 29, native of Michigan; height, 5 ft. 11 in.; weight, 150 pounds; family history, consumption on the side of the father, whom he resembles. For several weeks he had been working among the cedar brakes on the Colorado river, where he contracted chills and fever, which soon compelled him to quit work.

When seen, Aug. 26th, he was delirious, very restless, with marked opisthotonos, and complained of constant and severe pain in the back part of the head and neck. His temperature in the axilla was 106° F., pulse 120, resp. 32, spasmodic. He was given potassii brom. gr. x, tr. aconit. gtt. iij, spt. æth. nit. f℥ss, every two hours until fever abated; then cinchonia alkaloid gr. xx, in four doses, one to be taken every four hours. On the following day I saw the patient, and from that time on the case was treated by my brother and myself in company.

During the night of the 26th, the fever left him

and returned on the afternoon of the 27th; the temperature reached 105°, but there was no opisthotonos and but little delirium. The urine was scanty and very dark, coffee-colored, and much pain was felt in the lumbar region. He was given spt. æth. nit. a teaspoonful every three hours for two days, and the same amount every six hours for several days longer, the urine gradually becoming normal in quantity and color. During the night of the 27th he took quinine gr. xxx, with pulv. doveri gr. x.

There was no return of the fever, but the patient was greatly prostrated so that he could scarcely sit up in bed. He was stupid, and talked incoherently much of the time during the 28th, still complaining of pain in the head. On the morning of the 29th his left jaw began to swell, and by noon the whole left side of the face and neck was much swollen and very painful, especially in the parotid region. There was also intense pain in the left ear, and the patient was continually turning from side to side and crying out. Morphine in doses of gr. ¾ to gr. j, every four hours, and local applications of vinegar and hot fomentations, gave only slight relief. On the 30th, the right parotid region became inflamed, and the swelling on that side was soon as great as on the left, reaching an enormous extent on both sides, extending down the neck to the shoulders and backwards beyond the mastoid region, so that the auricles were thrust upwards and forwards, giving a very grotesque appearance to the sufferer's countenance.

The prostration was so great, and the rigidity of the jaws such, that food was administered with difficulty. He was given half an ounce of brandy in egg-nog or milk-punch every two hours for three days, and at longer intervals, alternating with animal broths, for about two weeks. Quinine gr. v was also given night and morning from Aug. 28 to Sept. 8. His tongue was enlarged and heavily coated, mouth very dry, and hearing much impaired. On Sept. 1st there was a discharge of thin, ichorous pus from the left meatus auditorius externus. On the 2d, a similar discharge took place from the right, and fluctuation was noticeable just back of the lobe of each ear. He also had a slight chill, and night sweats set in, for which he was given 20 drops of aromatic sulphuric acid three times a day for four days.

On Sept. 3d, the abscess on the left side was lanced at a point half an inch below and behind the lobe of the ear, and the day following that on the right was opened at a corresponding point, in both cases giving exit to a large quantity—an ounce or more—of thick yellow pus mixed with a substance resembling the white of egg. From this time on the pain was much less severe, and the morphine was discontinued, but, as the patient kept calling for it at frequent intervals, to pacify him a number of powders were made out, consisting only of wheat flour, labelled "morphine," and after taking one of these he always seemed to be greatly relieved. His bowels were inclined to be constipated, but an action was procured about once in two days by the use of small doses of sulphate of magnesia.

On the 8th, he was put upon the following tonic which he continued to use for three weeks.

℞.—Tr. nucis vomicæ, . . . fʒiiss.
Tr. cinchonæ, . . q. s. ad. fʒiv.—M.
S. A teaspoonful four times a day after eating.

An attempt was made to apply compresses to the sides of the neck in order to secure thorough evacuation of the abscesses and prevent burrowing of the pus, but the patient would not tolerate any kind of dressing, pulling them off as fast as they could be applied.

On the morning of the 10th, the openings made by the lancet, which had become considerably enlarged by sloughing, were found occluded by what the attendants thought to be "proud flesh." This was removed by pulling it out with forceps, and was found to be the parotid glands almost entire, the superficial portions having sloughed away so that only a few fibres of tissue held them in place. The left gland came out easily, the right required a little cutting to detach it from its connections. Both presented a similiar appearance, being somewhat spongy and having deep sulci, apparently where large bloodvessels or nerve-trunks had passed through their substance, and the right gland still showed something like the outlines of the natural shape, but the left was not larger than the first joint of the thumb, and its true character could be made out only by its position and its internal structure.

After the removal of the glands as much as three fluidounces of thick pus escaped from each cavity. There was no hemorrhage nor has there been any paralysis, so that no important nerves or bloodvessels could have been involved. No other glands were implicated nor was there any burrowing of pus. The discharge from the ears ceased in a few days after the glands came away, the only treatment directed to them having been a daily syringing out with warm water.

On September 13th, he was given sixty granules containing one-tenth of a grain of calcium sulphide each, of which he took three per diem until all were used. He recovered strength slowly, the wounds remaining open until about the middle of October.

By the 10th of November he was entirely well, except slight stiffness of the jaws. He then complained of having more "heart-burn" than he used to have, and said that his mouth seemed dryer than natural. A careful examination with a fine probe showed Steno's duct to be entirely closed on the left side, and to consist only of a *cul-de-sac* about one-fourth of an inch deep on the right.

At the present time, November 25th, his gastric trouble has disappeared and his mouth has regained its natural amount of moisture, doubtless through an increased activity in the remaining salivary glands. The loss of the parotid glands has caused no deformity beyond a slight depression and cicatrix at the site of the openings made by the lancet. The hearing is normal.

This case seems to be unique. Flint, *Practice of Medicine*, p. 401, says that parotiditis is an occasional complication of typhus and typhoid fever, and that in such cases considerable sloughing of the

areolar tissue frequently occurs, but neither he nor any other writer, so far as I know, mentions loss of the gland as a possible result.

MEDICAL PROGRESS.

CUTANEOUS MANIFESTATIONS OF MALARIA.—PROF. VERNEUIL and DR. A. MERKLEN give the following conclusions in a paper with the above title:

1. Herpes is a frequent manifestation of malaria.

2. It may precede the access of the intermittent fever, occur during any one of the three stages of the attack, or after the sweat. There is consequently no etiological correlation between herpes and the fever, in spite of their frequent coincidence.

3. Malarial herpes possesses no special characteristics. Its most frequent seat of occurrence is on the face, and while usually discrete, it may occasionally become confluent.

4. The black crusts and vesicles of herpes are associated with the grave and pernicious forms of malarial fever.

5. Exceptionally, malarial herpes occurs under the form of zona.

6. In all forms, malarial herpes may be preceded or accompanied by vaso-motor troubles, disturbances of the sensibility of the skin in its neighborhood, and may perhaps indicate a nervous origin of the complication.—*Ann. de Dermatol. et de Syphilog.*, November 25, 1882.

THE COMPLICATION OF LABOR WITH OVARIAN TUMORS.—DR. R. LOMER, of Leipzig, contributes a paper on the above subject to a recent number of the *Archiv für Gynäkologie*. His conclusions are based upon the collections of cases made by Playfair and by Jetter. We shall only quote those which relate to treatment. But we may remark, first, that the tumors in the cases in question are always small, for the reason that large tumors are commonly found out before labor begins; and it is obvious that only a tumor small enough to be contained in the pelvic cavity can obstruct labor. Dr. Lomer's first practical rule is one of which the wisdom is obvious. It is, that in labors complicated with ovarian tumor, interference should not be too long delayed. 2. In all cases an attempt should be made to push the tumor out of the way, above the pelvic brim. 3. If this cannot be done, the puncture of the cyst should be the next alternative. 4. Should the contents of the tumor be too viscid to flow through a canula, a free incision should be made into the cyst-wall. Dr. Lomer remarks that from this measure to the pulling down of the cyst, ligature of its pedicle, and removal of the tumor—that is, vaginal ovariotomy—would seem but a step. Nevertheless, the latter operation has never yet been done, lack of time usually having been a sufficient obstacle. When the cyst has once been emptied, vigorous pains have forced down the child, and so put a stop to the operator's further proceedings. 5. All further attempts at emptying the tumor are dangerous, and should not be attempted. 6. When the tumor can neither be pushed up nor diminished in size, the choice must be made, according to the peculiarities of each case, between perforation and Cæsarean section.—*Medical Times and Gazette*, December 16, 1882.

A DOUBLE SUPERIOR VENA CAVA.—DR. H. REX reports such a case in which the right superior vena cava was formed by the union of the right common jugular and axillary vein, and was joined at the angle of union of these two vessels by the right vertebral vein. It passed in front of the trunk of the innominate artery and pulmonary vessels into the right auricle. The left superior vena cava could clearly be separated into a vertical and a horizontal portion; the vertical portion was formed by the union of the left common jugular, axillary, and vertebral veins, and passed backwards in front of the carotid and subclavian arteries, and in front of the ligamentum arteriosum and pulmonary vessels to the left side to reach a position behind the left auricle. The transverse portion was then bent on itself in the left auriculo-ventricular furrow, and entered the right auricle below the left pulmonary veins.—*Centralbl. f. die med. Wissen.*, November 18, 1882.

CHOREA AND RHEUMATISM.—DR. THOMAS R. FRAZER, F.R.S., narrates a case of chorea which shows in a most marked manner the relations between chorea and rheumatism. This relationship has long been recognized. First referred to by Bouteille and Berndt, it was afterwards acknowledged by Copland, Scudamore, Abercrombie, and more recently stated in the exact form of statistical enumeration by Hughes, Kirkes, Sée, Roger, and many other physicians. Its existence is now generally admitted, although occasionally it is regarded as a mere coincidence, the relationship being relegated even to the trivial bond which connects any depressing agent with the production of a disease. A case such as that which he at this time described, undoubtedly affords very strong support in favor of a causal relationship. It affords supports from two aspects of the case—from its time of occurrence, and from the results of the treatment. The former is so significant, in the first occurrence of the choreic disease as well as in its subsequent appearance during a relapse of the rheumatic fever, that it is impossible to overlook it. The latter has, in addition to its equally clear significance, the further value that it constitutes a description of evidence, the quantity of which, so far as he knew, was very limited. He referred for evidence of probably a similar description to the fact that some cases of chorea have been treated with success by Dr. Weir Mitchell and Dresch with the same remedy that produced such striking results in his patient. The details of these cases he has not learned, and he therefore cannot tell if they illustrate in so clear a manner the relationship between rheumatism and chorea by showing that the most certain remedies for the former disease with which we are acquainted—the several compounds of salicyl—may exert a like curative action upon chorea. It would be important to still further test this point by treating a number of cases of chorea by salicyl compounds, and especially cases where the chorea does not present so close a relationship to the rheumatic affection as to suggest the possibility of its being a mere symptom of rheumatism. This view is, indeed, one that has been already adopted. Its existence led Dr. Frazer to dwell upon the importance of avoiding any all-pervading opinion in regard to this disease. Nothing will appear more obvious, when the known facts in regard to chorea are considered, than that it is far from being the product of any single pathological condition. There is now much evidence to show that, at times, it is accompanied by a lesion of the corpus striatum, at others of the medulla oblongata, at others of the medulla spinalis, at others of some part of the peripheral nervous system, at others of the heart, and at others by no lesion that can be discovered in any of these situations.

Dr. Frazer can scarcely hazard an opinion as to its production in the present case. The embolic theory is here entirely inapplicable, because of the rapidity

with which the disease disappeared on two separate occasions. The mere existence of cardiac disease, of rheumatic origin, seems equally insufficient to afford an explanation. It existed before the chorea, it has persisted after its disappearance; and, further, the chorea was rapidly cured by salicylates, whereas salicylates do not directly affect the occurrence or the course of cardiac disease in acute rheumatism. A reflex influence, originating from the inflamed joints, cannot account for the chorea, as it often appears in rheumatic patients, while no inflammation of joints is present; and, in our case, the joints were on several occasions acutely inflamed, without a reappearance or an exacerbation of the chorea. Such a case would almost indicate that chorea may be a mere manifestation of acute rheumatism, in the same sense as the joint or heart affections, a mere complication of that disease, produced, it may be, a rheumatic inflammation of some portions of the central nervous system, whether true nerve-substance or surrounding media.—*British Medical Journal*, December 9, 1882.

TREATMENT OF DIPHTHERITIC SORE THROAT.—DR. W. ALLAN JAMIESON has used M. Barff's boro-glyceride in conjunction with salicylic acid, in the treatment of this disease, and the results of this combined treatment have been highly satisfactory. The saturated solution of boro-glyceride in glycerine causes no pain when painted on the inflamed, ulcerated, or sloughy mucous membrane, but its use is immediately followed by relief to the symptoms. The disease, however, causes blood-poisoning, evidenced by the fever and the albuminuria, and against this the salicylate of sodium acts energetically, as has been seen in the recorded cases, which are but examples of what occurred in many others. With this treatment there have been no recurrences, no formation of abscesses; recovery has in all been rapid and satisfactory. In conclusion, he recommends a trial of the boro-glyceride in glycerine in ulcers of the mucous surfaces, as of the cervix uteri. The treatment has another advantage over any form of gargle, that by painting the inflamed surface we keep it at rest, instead of, as in the act of gargling, causing painful and injurious movement by the unavoidable contraction of the muscles of the palate and pharynx. In the case of children, even, little difficulty was experienced when they found not pain, but comfort, followed its use.—*Edinburgh Medical Journal*, December, 1882.

A NEW MOXA.—Under the name of "*crayon-feu*," DR. MOSES describes a preparation made as follows: Charcoal powder, 30 grammes; nitrate of potash, 4 grammes; pulverized iron, 5 grammes; benzoin, 1 gramme. The whole to be made up, with some adhesive substance, into forty crayons. He so obtains a hard preparation, which is easily inflamed by a match, and which he proposes for the cauterization of poisoned wounds, and when the actual cautery is required.—*Gaz. Hebdomadaire*, Dec. 1, 1882.

MYOSITIS OSSIFICANS.—At a recent meeting of the Vienna Medical Society, Prof. Podrazki exhibited a soldier affected with the rare condition which has been termed myositis ossificans. Four weeks previously the man had applied for treatment, on account of an intense inflammation of the muscles on the front of the right upper arm, apparently set up by severe gymnastic exercise. The muscles were large, hard, and uneven, and the elbow-joint was fixed in flexion. The hardness was removed, and some increased mobility was obtained, by massage and the application of cold. At the end of two weeks a hard, round, movable tumor developed in the flexor of the elbow, which was evidently due to an ossification of the brachialis anticus. At first it was movable, the upper part appeared to be cartilaginous, and it was evidently not connected with the periosteum. Podrazki has seen, in the course of nineteen years, two cases in the practice of Pitha quite similar to this in their characters. In those two cases neither iodide of potassium, nor any other treatment adopted, had any influence. In a discussion which followed, Prof. Weinlechner stated that he had twice seen similar small spots of ossification in the muscles on the front of the leg, due, in each case, to a traumatic cause. Kundrat expressed the opinion that some supposed exostoses on the thigh proceed from muscles. Their form and seat correspond to certain muscles. Their greater frequency in men, and especially in muscular individuals, suggests that their origin is traumatic. They constantly become adherent to bone in the course of their growth, and hence are commonly thought to be primary exostoses.—*Lancet*, December 16, 1882.

MALIGNANT ŒDEMA.—BRIEGER and EHRLICH, working in Frerichs' clinic, have recorded (*Berliner klin. Wochen.*, No. 44) two cases of an affection associated with typhoid fever in which they recognize a disease that in animals has been called "malignant œdema." This malady is an infectious one, and dependent on, or at all events associated with, a bacterium which has sufficiently well-defined characters. The exciting cause of the malignant œdema in the authors' cases was a hypodermic injection of a musk solution administered as a stimulant to overcome the state of profound collapse into which both patients had fallen. Much swelling of the subcutaneous tissue, with emphysematous crackling and discoloration of skin at the site of the former injection (in the thigh in both examples), coming on in forty-eight hours, were the features descriptive of the malignant œdema. The authors remember to have met with another example of the affection in a case of diphtheria, but here there was no obvious exciting cause, and the emphysematous and œdematous conditions developed about the front of the chest. Inoculations of some of the fluid from the diseased thigh of the typhoid patients, performed on rabbits and guinea-pigs, brought about the usual characters of malignant œdema at the focus of vaccination, and the animals died in a few days. The existence of the septic vibrios was proved by the microscope (after the usual method of preparation), both in the fluids of the patient and in those of the animals experimented upon. Brieger and Ehrlich regarded their patients as suffering from a *mixed infection*, the virus of enteric fever and that of malignant œdema being both present at the same time. There are many facts in medicine which might be looked upon as demonstrating the predisposition which one complaint establishes for another—noma following measles and other acute specific diseases, tuberculosis after measles and whooping-cough, joint-suppurations after typhus, septicæmia after scarlatina, and many other examples. In the language of bacterial pathology, the human garden, by the action of one bacillus, is prepared and fitted for the growth and development of another micro-organism, which in the normal state of health would not have found so suitable a nidus.—*Medical Times and Gazette*, December 16, 1882.

TREATMENT OF FETID BRONCHITIS.—DR. E. LANCEREAUX recommends the hyposulphite of sodium in the treatment of fetid bronchitis; he combines it with the syrup of eucalyptus, 4 grammes of the hyposulphite to 30 of the latter. He has seen the greatest improvement in five cases treated on this plan.—*Bull. Gén. de Thér.*, November 30, 1882.

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THE EXISTING STATE OF OUR KNOWLEDGE CONCERNING THE BACILLUS MALARIÆ.

HOWEVER erroneous and ill founded were the conclusions of Dr. Salisbury, in the matter of the micro-organism he announced as the cause of malaria, his observations must be acknowledged to have become a part of the history of a subject which promises to be of vastly greater interest and importance than was for a long time anticipated. The names of other observers since Salisbury, but before the announcement in 1879 of the discovery by Klebs and Tommasi-Crudeli of a bacillus which is the cause of malarial disease, have been more or less associated with a micro-organism of similar properties, but no decided impression was made until the last-named published their results.

These observers found in the earth of malarial districts, in Italy, numerous shining oval and mobile spores, .95 of a micro-millimetre in the longer diameter. They were able to cultivate these spores in the animal body as well as in culture experiments, and the animals infected by them exhibited not only the clinical course of malarial disease as seen in man, but also the post-mortem appearances; while the bacillus was also found in the blood of such animals, taken after death. The spores develop in the animal body, as well as in culture experiments, into long threads, which are at first homogeneous, but later divide, while new spores develop in the interior of the segments. The position of the spores which are found either at the poles, or in the middle of the segment, serves as a mark of distinction between this and other pathological bacilli.

Following Klebs and Tommasi-Crudeli, Marchiafava and Cuboni, in Italy (*Archiv für Experimentelle Pathologie*, vol. xiii.), studied the blood of men ill with malaria. In this they found spores and bacilli which they declared to be identical with those described by the former. The spores included in the white blood-corpuscles were sometimes so numerous as to seem to fill them completely. Similar studies on malarial patients by Lanzi, and again by Peroncito, led to the same conclusions.

Succeeding these, Marchand published in Virchow's *Archiv*, vol. lxxxviii., p. 104, April, 1882, some observations really made in 1876, whence he concluded that there exists in the blood, in the cold stage of intermittent fever, mobile and flexible rods presenting slight swellings at their ends, and sometimes also at the middle. These end-swellings he thought also might be of the nature of spores.

More recent still are the elaborate experiments of Prof. Ceri, of Camereno, Italy, published in the *Archiv für Experimentelle Pathologie*, vols. xv. and xvi., 1882. These consisted of culture experiments with organisms found in malarial and other soils, of experiments on animals, and culture experiments with quinine. They resulted in proving that the spores could be cultivated—Ceri applying the term *natural germs* to those found in the atmosphere and soil, and *artificial germs* to those which result from their culture; that animals could be infected by their injection into the blood, though to a less degree by the cultivated than by the natural germs, the former growing weaker in successive generations; and that the infecting properties could be retarded by the application of heat to culture fluids, and the introduction of quinine into them, certain degrees of the former, and strengths (1:800) of the latter making the culture of the spores impossible and arresting the putrid fermentation induced by them. The practical application of these facts is self-evident.

Finally the opportunity has recently been presented to Dr. Franz Ziehl to test these results clinically (*Deutsche Medizinische Wochenschrift*, Nov. 25, 1882) in three typical cases of malaria, in all of which the spleen was enlarged. In all three the bacilli above described were found in the blood taken from any part of the body by the prick of a needle, and examined in the fresh state, or dried in a thin layer upon a cover glass, by simply passing the latter over a flame. These have been preserved by Dr. Ziehl for three months without undergoing any change.

The bacilli thus observed were of different lengths, but usually were from one-fourth to the entire diameter of a red corpuscle. The majority of those measured were about 4 micro-millimetres long and .7 broad. Their ends were swollen and roundish.

The ratio of the diameter of the former to the latter varied, being sometimes as 1 to 2, sometimes more and sometimes less. They presented active flexions and movements among the red corpuscles, their motion being so slow that they often remained for half an hour in the field of view afforded by a Zeiss's objective F, and ocular I (about 410 diameters). With the same amplification there was present for the most part but a single bacillus in one field, and never more than three. In two of these cases *the use of quinine was followed by prompt relief and the disappearance of the bacilli from the blood in seven and nine days* respectively. The third case improved under the use of Fowler's solution but, unfortunately, no further examination of the blood was made.

Subsequently, Dr. Ziehl examined the blood of twenty-five persons, healthy or ill of other conditions than malaria. In *only one of these twenty-five* did he find bacilli, and these in considerable numbers. The man was a diabetic, in whom the disease clearly succeeded upon exposure. There was no history of malaria, but Dr. Ziehl, remembering that diabetes mellitus has been occasionally traced to malaria, put his patient upon quinine. The daily excretion of sugar, which had been reduced by opium and diet to one-half the quantity noted on admission, was not further reduced, but the *bacilli disappeared from the blood in eleven days* after the treatment was instituted. It would seem, therefore, that quinine acts as a parasiticide, in some way destroying the bacillus, or, at least, interfering with its development.

Should these observations be confirmed, and it must be admitted that the whole chain of evidence is much more conclusive than that adduced in favor of a *bacillus tuberculosis*, the whole mystery of the efficacy of quinine in the treatment of malaria would be solved. It is evident, also, that the question is one of much easier solution than that of the bacillus tuberculosis, and we predict a speedy settlement of it.

With a view of completing the presentation of the matter, we must mention a microbe claimed to have been discovered in the blood of persons suffering from malarial fever by Richard (*Comptes Rendus*, vol. xciv. p. 49), a French investigator, and named by Laveran *oscillaria malariæ*. This infests the red-blood disk, and its development is said to be analogous to that of a parasite in the lentil. We condense from our contemporary, the *Philadelphia Medical Times*, the annexed description of the microbe. In the blood of malarial subjects will be found some corpuscles presenting on their periphery a minute light and perfectly round spot, but which are otherwise exactly like the normal corpuscles. In a further stage of development the spot is larger and surrounded by a wreath of fine black nodules; around them hæmoglobin—recognized by its yellowish-green color—is deposited in the form of a ring, which diminishes as the microbe grows, until nothing is left of the cell except a marginal zone, whence the hæmoglobin is completely absent. Thus the microbe completely occupies the corpuscle, being still surrounded by the ring of black nodules. Finally, the fully developed oscillaria, which possesses several scarcely visible prolongations, perforates the membrane and escapes into the liquor sanguinis. Occasionally the prolongations, or fibres, which have a power of motion, alone perforate the membrane, and the microbe itself remains within the cell wall. Sometimes, also, the fully-grown microzyme is seen to move, and its fibres oscillate rhythmically, as "thin twigs would when held by their thicker ends and rapidly shaken." This movement continues about an hour, when it ceases, and only the dead body of the parasite is left. The dead parasites lose their shape, the ring of pigment is dissolved, and the whole appears as a gray mass, including a few black nodules, which are subsequently liberated and rapidly absorbed by the leucocytes.

Such is the somewhat fanciful description by Richard of the *oscillaria malariæ* which, he says, he was able to demonstrate in the blood of all persons suffering from malarial infection who came under his charge, while the severity of the disease was proportionate to the number of microzymes found in the blood. Kelsch and "a few other observers" are referred to as having noted these same phenomena. We have no critical comment to make upon these organisms other than that the changes described are strikingly similar to certain ones undergone by the red-blood corpuscles in the process of crenation and drying.

THE MEANING OF NYCTALOPIA AND HEMERALOPIA.

MEDICAL terminology, and particularly that of ophthalmic surgery, is often accused of being formidable, and it is not too much for those who undergo the labor of mastering it to expect that they shall be in possession of terms that are at least definite, if not etymologically correct.

Unnecessary changes are of course to be avoided, and if the name given to a disease or symptom can claim the universal consensus of medical writers, if it involves no absurdity and is free from ambiguity, the wisdom of tampering with it may be doubtful, even if it cannot be defended on either scientific or literary grounds. But if its etymology is more than questionable, its application absurd, and its use confusing, certainly the sooner it is consigned to the limbo of obsolete words the better. Nyctalopia and hemeralopia seem to be much in this position.

If we wish to record the fact that a patient's right leg is paralyzed, we do not do so by stating that his left is vigorous, and though we may indicate that the right half of his field of vision is gone by stating that the left half remains (*i. e.*, that he has left hemiopia, or half vision); the term hemiopia is usually translated as "half blindness," and of late we have the choice of the more rational expression hemianopia or hemianopsia, which is coming into general use. Not so with the writer who wishes to state, in one rather learned looking word, that his patient cannot see at night, or that he cannot see so well in daylight as at dusk. He is not allowed the benefit of a translation "by contraries," or the choice of another word, but must, perforce, commit a verbal absurdity and write down "hemeralopia" or "nyctalopia."

Among modern authors the use of the former term to express night-blindness and of the latter to express day-blindness is almost universal. However inconvenient and confusing these terms may have been, we have consoled ourselves with the belief that their etymology was settled, and their use sanctioned by an antiquity extending back into the mythical ages of medicine. Elaborate articles, however, by Drs. Greenhill and Tweedy, in the last two numbers of *The Royal London Ophthalmic Hospital Reports*, show that the etymology of these words admits of an amount of learned philological discussion sufficient to bring doubt to the most conservative mind, and that the use of them in their present sense is a comparatively modern and probably accidental innovation.

These authors say that Oribasius, Palladius, Aëtius, Alexander Trallianus, Paulus Æginata, Joannes Actuarius, and a host of others use nyctalopia in the sense of night-blindness. Galen, who distinctly explains nyctalopia to mean night-blindness in one of his works, has been accused of taking the opposite side in two others, but the latter are apocryphal. In fact, up to the middle of the seventeenth century, Hippocrates only can be cited as using nyctalopia in the sense of night-sight. About this time, the Hippocratic works were more generally consulted, and the passage—"Those who see at night, whom we call nyctalopes," etc., determined the meaning of nyctalopia, while hemeralopia came into use to indicate the opposite kind of sight. It is shown, however, that the description of the etiology, history, and treatment of the disease, which follows this passage, corresponds to night-blindness rather than to day-blindness; and a revised edition of Hippocrates is produced in which the word οὐχ is restored, making the passage read: "Those who do *not* see at night," etc., and undermining the faith that has inspired many generations of medical authors.

Drs. Greenhill and Tweedy have done a great service to medical literature by determining, at the expense of much labor and learning, the true meaning of these words; but we fear that it will hardly be possible while they remain in use "to put an end to the confusion which has existed for considerably more than a thousand years." It would probably be easier to change the terms than to reverse their accepted meaning. As hemianopsia and hemiachromatopsia have definite and unmistakable meanings, might not some such words as nyctanopsia and hemeralanopsia be made equally useful?

AN IMPORTANT DECISION AS TO QUARANTINE FEES.

A DESPATCH dated New Orleans, January 2, states that a perpetual injunction restraining the Board of Health of that city from collecting quarantine fees, has been granted by the District Court on the application of the Morgan Railroad and Steamship Company. Judge Monroe, in delivering the opinion of the Court, concluded as follows: "It is remarkable, in view of the fact that the convention which adopted the Constitution of the United States distinctly determined, after the most elaborate and earnest discussion, that the States should not be allowed without the consent of Congress to levy duties on tonnage for any purpose whatever, that it should still be considered a matter of discussion as to whether such duties may not be imposed for the support of a police or quarantine established by a State for the protection of the health of its citizens. There is no reason for requiring that the quarantine service should be supported by those who are under no legal obligations to do so."

As the quarantine fees are the chief source of revenue of the New Orleans Board of Health, which receives no appropriations from the city or the State, this decision to a large extent deprives it of funds. The opposition of the New Orleans Board of Health to the National Board, and especially to the Ship Island Quarantine, has been no doubt influenced to a certain extent by the fear that its revenue from quarantine fees might be diminished if vessels were allowed to perform quarantine elsewhere than at its own station, but it may now be able to take a more impartial view of the matter. This decision, if sustained by the superior courts, before which it will no doubt be brought in some way, affects all ports of the United States where quarantine fees are charged, and it would seem as if under it one or two things must follow: *i. e.*, either the States or cities must appropriate the necessary funds for maintaining these quarantine inspections, or the general Government will be asked to assume the expense, and take charge of the matter.

Not having the full text of the decision before us,

it does not seem worth while to comment further on the matter at present, but it is possible that this is the first step towards a really national system of quarantine.

THE INFLUENCE OF ARSENIC ON THE FORMATION OF SUGAR.

THIS subject has been studied by M. LE DR. LONGEVIALLE, both by physiological experimentation and by clinical observation. He has ascertained that the saccharine condition of the urine produced by puncture of the floor of the fourth ventricle, is very greatly diminished by the simultaneous administration of arsenic. He has further observed that in cases of diabetes the quantity of sugar is equally diminished by the administration of Fowler's solution. This medicine he administers in doses which, to our notions, are rather extreme, giving from ten to thirty drops daily, and increasing the quantity up to the tolerance of the stomach. The result is striking. Not only is the quantity of sugar greatly reduced, but the simultaneous waste of the nitrogeneous materials in the form of urea is also much diminished. With the diminution in the quantity of urea and of sugar, a notable decline in the quantity of the urine passed also takes place.

We are able to confirm these observations as regards the power of arsenic to check the formation of sugar, and the excessive production of urea, in cases of glycosuria. Although Longevialle has reduced to scientific expression the utility of arsenic, it must be admitted that it has long been known that arsenic is a useful remedy in diabetes. The clinical fact now receives scientific confirmation, but the fact has, nevertheless, long existed.

THE Pseudo-bacillus Tuberculosis of Schmidt, we feel, should be the term applied to the object of the studies of Dr. Schmidt, of New Orleans. That this object is a fat crystal derived from the fat so abundantly present in the sputum of tuberculosis, as well as in tubercle itself, there is every reason to believe. But that this is the object which has been called the bacillus of tuberculosis by Koch and others is not at all likely. No further proof of this ought to be required than the demonstration by Dr. J. Gibbons Hunt, at the College of Physicians of Philadelphia, last Monday night. That the pseudo-bacillus is a crystal, is proved by the fact that it *polarizes* under the microscope, while the true bacillus does not polarize. These different properties were shown side by side. Otherwise the two objects are similar, but Dr. Schmidt has mistaken the crystal for the real thing, which it closely resembles.

IT is important to publish widely the fact that in Illinois one Lambrecht has recently been detected in an attempt to personate Dr. Heinrich Andreas Lüders, a graduate from the University of Göttingen in May, 1866, who died in November, 1878. Lambrecht, who is a barber by trade, became in some way possessed of the diploma of the dead man, and attempted to register under it; and although, by reason of suspicious circumstances connected with the application, the required certificate was held back for some time by the Illinois State Board of Health, it was finally obtained in two years from the date of application, and he began to practice in Collinsville, Ill. The fraud was discovered by correspondence with the dean of the medical faculty of Göttingen, but not until the man had sacrificed a mother and infant in the attempted *rôle* of obstetrician. The scoundrel escaped, however, before he could be arrested, and may attempt the same game in some other State, where requirements of registration are less rigid, or do not exist at all.

Attempts at personating others through diplomas acquired by theft or otherwise are not infrequent, and suggest a rigid examination as to the identity of the individual with the name on the diploma. In this instance, the unscrupulousness of the man in forging letters of recommendation enabled him to deceive the Secretary of the Board. Had an affidavit before a justice of the peace, properly executed, been required in addition to the letters of recommendation, another obstacle to the success of the fraud would have been interposed.

THE new year has brought with it the usual number of alterations in our exchanges.

The New York Medical Journal has been converted from a monthly to a weekly, of 28 octavo double-columned pages, with the usual diversity of matter. The first number contains for its leading article Dr. W. B. Carpenter's initial lecture on Human Automatism, a full abstract of which was laid before our readers in our issue for December 2d. The journal remains under the editorial control of Dr. F. P. Foster.

The Sanitarian likewise becomes a weekly publication, and the first number consists of sixteen handsome double-columned quarto pages. It remains under the able editorial control of Dr. A. N. Bell, who is assisted by Dr. T. P. Corbally. The opening number contains papers by Drs. Cabell, Bowditch, Peters, and others.

The Medical Record, with the new year, appears in new type, a greatly improved dress, and a lengthened page, and it announces its intention of availing itself of all the resources of modern medical journalism.

The Detroit Clinic and *The Michigan Medical News* pass under the control of Mr. George S. Davis, and will be combined and published hereafter under the title of *The Medical Age.*

THE cost to the Government of the star route trials has just been made public, and we learn that Mr. George Bliss has been paid up to December 1st $40,660.34, and the total sum paid to special counsel at that date was about $70,000. Judge Porter and Mr. Davidge each received for his services in the Guiteau trial $10,000, and Mr. Edwards Pierrepont was paid $3,099.25 in the past year for his services in the discontinued Tilden income-tax suit. These sums have been paid without eliciting a word of comment from the same Congress which, with considerable appearance of virtue as well as economy, denounced the extravagance of the sum it was proposed to pay the four surgeons who were in attendance upon President Garfield, and then created the Board of Audit which subsequently awarded them the total sum of $20,500. We commend these figures to the thoughtful consideration of the profession and the public.

SOCIETY PROCEEDINGS.

COLLEGE OF PHYSICIANS OF PHILADELPHIA.

Stated Meeting, January 3, 1883.

THE PRESIDENT, W. S. W. RUSCHENBERGER, M.D., IN THE CHAIR.

DR. JOHN B. ROBERTS read a paper on

HEART-PUNCTURE AND HEART-SUTURE AS THERAPEUTIC PROCEDURES.

It is more than probable that in a few years puncture of the heart-wall (cardicentesis), with direct abstraction of blood by aspiration, will be recognized as the best treatment in cases of greatly dilated or much distended right heart, with intense pulmonary engorgement; and that incision of the pericardium, with suture of the heart muscle, will be accepted as proper in cardiac wounds. Hence these latest novelties in cardiac surgery deserve the attention of the Fellows of the College.

That punctures of the heart are comparatively harmless has been well known to many for some years. In 1872, Roger, while performing pericardicentesis on a child with pericardial effusion, thrust the needle into the right ventricle and withdrew about 6¼ Troy ounces (200 grms.) of pure venous blood. The boy, who was aged five years, became pale, sweated, and had an imperceptible pulse. The withdrawal of the pericardial fluid, accomplished prior to the heart injury, was beneficial; and the cardiac puncture did no permanent mischief, for the patient recovered. Death occurred five months later from long existing dilatation and valvular disease of the heart (*Bull. de l'Académie de Médecine*, 1875, p 1276).

In Hulke's case (*Trans. Clinical Society of London*, viii. p. 169), a woman with pleuro-pneumonia was supposed to have large pericardial effusion, and a trocar was introduced through the fourth left intercostal space. Nothing escaped except a drachm of venous blood, after which the patient seemed relieved of dyspnœa. She died four weeks later from a complication of diseases, and the autopsy revealed cardiac dilatation and valvular changes.

I have said elsewhere (*Paracentesis of the Pericardium*, 8vo., Philadelphia, 1880), in commenting upon this case: "The abstraction of blood seemed to relieve the distended heart much better than phlebotomy would have done, as was evinced by the diminution of threatening symptoms and the decrease of the area of dulness."

Cloquet, Bouchut, Legros, and Onimus have also observed the apparent innocuousness of wounds of the heart made by capillary trocars. Steiner found, ten years or more ago, that electro-puncture needles could be quite safely introduced into either ventricle, provided they were at once withdrawn (*Med. Times and Gazette*, May, 1873, p. 492, from *Langenbeck's Archiv für klin. Chirurgie*).

It has been considered less safe to puncture the auricles; but the interesting paper of Dr. Benj. F. Westbrook, just published in the *Medical Record* for December 23, 1882, seems to show that our fears are as unfounded as were those of our predecessors in regard to ventricular puncture. It is, in truth, to call attention to his case of harmless *intentional* cardicentesis and to his researches in the surgical anatomy of the operation, that I have been led to refer to the corroborative evidence of the cases mentioned above.

I have with much satisfaction, as have many others, done venesection at the bend of the arm for the temporary relief of the distressing symptoms of dilated heart, and for the dyspnœa due to the pulmonary engorgement of acute pneumonia. If, however, a few *drachms* of blood drawn directly from the heart give the relief that could only be afforded by taking a similar number of *ounces* from the veins of the arm, it seems proper to adopt the former measure. The subsequent circulatory depression from anæmia would undoubtedly be less than after the latter operation.

It is manifestly necessary, however, to determine that cardicentesis is innocuous before it can take the place of venesection. The above-mentioned cases and Dr. Westbrook's experience tend to show that such is the fact.

Dr. Westbrook believes that the proper place to perform the operation is in the third costal interspace close to the *right* edge of the sternum. This situation enables the operator to tap the right auricle without injuring the right internal mammary vessels, and with little danger of striking the tricuspid valve. My own preference would be to perforate the ventricle of the right heart by introducing the needle through the fourth interspace, about one and a half or two inches to the *left* of the median line of the sternum. Dr. Westbrook's opinion, however, is entitled to more deference than mine, because he has studied the subject with special reference to cardicentesis, while my special investigations have been limited to the consideration of pericardicentesis.

Further experimentation in heart-puncture for the relief of cardiac distention and pulmonary engorgement is requisite, but it is probable that it will soon become a well-recognized surgical procedure in selected cases. Pericardicentesis has already taken that position, and there is no reason to believe that cardiac surgery will stop its march with the demonstration that the pericardium can be treated as the pleura.

In October, 1881, I read a paper before the Anatomical and Surgical Society of Brooklyn (*The Surgery of the Pericardium; Annals of Anatomy and Surgery*, December, 1881), in which I advised resection of the costal cartilage and incision of the pericardium for removal of foreign bodies in the pericardial sac; and at the same time said: "The time may possibly come when wounds of the heart itself will be treated by pericardial incision, to allow extraction of clots, and perhaps to suture the cardiac muscle."

It seems as if this time had now almost arrived, for Dr. Block has not only expressed a belief that death

can be averted in many cases of heart-wounds by simple incision of the pericardium to allow escape or extraction of the clots which cause pressure and death, but has also undertaken to demonstrate by vivisectal experiments that suture of the heart is a simple operation and requires but three or four minutes (*Amer. Journal of the Med. Sciences*, January, 1883, p. 276; from *Journal de Méd. de Paris*, Oct. 28, 1882; from *Gaz. Méd. de Strassbourg*, Oct. 18, 1882). He finds that opening of the right and left ventricles, and entire compression of the heart for the application of sutures, can be supported by rabbits for several minutes. During suturing he seizes the apex of the heart and draws the organ forward until the traction prevents the escape of blood from the wound. Sutures are then introduced, or the orifice closed by ligation. Even if cardiac pulsation and the respiration stop during this mechanical interference with the heart's movement, death, he asserts, does not necessarily ensue.

These experiments are even more important than the researches spoken of in regard to heart-puncture. I regret that as yet I have not been able to consult Dr. Block's original memoir, but I hope at a future time to do so, and perhaps to be able to report some investigations of my own which I desire to make in the same direction.

The following Fellows were then elected

OFFICERS FOR THE ENSUING YEAR.

President.—Alfred Stillé, M.D.
Vice-President.—J. M. Da Costa, M.D.
Secretary.—R. A. Cleemann, M.D.
Treasurer.—Chas. Stewart Wurts, M.D.
Curator.—Thomas Hewson Bache, M.D.
Honorary Librarian.—James H. Hutchinson, M.D.
Recorder.—J. Ewing Mears, M.D.
Censors.—Drs. Lewis Rodman, Edward Hartshorne, William Goodell, and Samuel Lewis.
Councillors (to serve three years).—Drs. S. Weir Mitchell and W. S. Forbes.

The following resolution complimentary to

THE RETIRING PRESIDENT

was unanimously adopted.

"*Resolved*, Inasmuch as the term of the retiring President of the College, W. S. W. RUSCHENBERGER, M.D., has just been completed under the limit prescribed by the by laws, the Fellows of the College hereby respectfully and cordially congratulate him upon the very successful termination of his official services, and upon the long-continued, constant, and faithful administration which has distinguished those services not only in the Presidential Chair, but in his unremitting, wise, and watchful attention to the interests of the College.

"They take especial pleasure in expressing the grateful appreciation of the College, and in recording the fact that throughout his nearly four years of service as Vice-President, and his subsequent four years of service as President, Dr. Ruschenberger, while holding always the pleasantest relations with his Fellows of the College, has never been obliged by ill health or other cause to absent himself from his post, either at the meetings of the College or of the Council."

NEW YORK ACADEMY OF MEDICINE.

Stated Meeting, January 4, 1883.

THE PRESIDENT, FORDYCE BARKER, M.D., LL.D., IN THE CHAIR.

The result of the

ANNUAL ELECTION

of officers for 1883 was as follows:

President.—Fordyce Barker, M.D.
Vice-President.—H. P. Farnham, M.D.
Recording Secretary.—W. H. Katzenbach, M.D.
Corresponding Secretary.—J. G. Adams, M.D.
Treasurer.—William F. Cushman, M.D.
Trustee.—G. M. Smith, M.D.
Treasurer of Board of Trustees.—Charles Wright, M.D.
Mem. of Com. on Admission.—E. L. Partridge, M.D.
Mem. of Com. on Ethics.—H. E. Crampton.
Mem. of Com. on Education—J. C. Dalton, M.D,
Mem. of Com. on Library.—F. R. Sturgis, M.D.

The scientific work of the evening consisted in the reading of a paper by DR. FRANCKE H. BOSWORTH, on

TUMORS IN THE NASAL PASSAGES.

By nasal passages, he said, was meant the whole of the physiological air tract from the nostrils to the lower part of the soft palate.

The most frequent form of growth is the adenoid in the vault of the pharynx. It consists simply in a hypertrophy of the glands normally found in that region. It is a rather common belief that adenoid tumor in the vault of the pharynx is a rare and obscure disease, whereas Dr. Bosworth had found it a most common affection. The point from which he wrote was that this was one of the conditions which gave rise to so-called nasal catarrah. Nasal catarrh, however, was to him a meaningless term. There were a number of different conditions which gave rise to discharge from the nose. In a very large number of instances the discharge is the result of a definite morbid condition, the removal of which results in radical cure of the disease.

Among the common causes or sources of nasal catarrh were mentioned in passing rhinitis hypertrophica, deviated septum, adenoid growths in the vault of the pharynx, nasal polypus, presence of foreign bodies, syphilitic ulceration or necrosis, and strumous ulceration. These were diseases which generally presented to the physician for treatment as nasal catarrh.

During the last twelve months seventy-five cases of these growths (adenoid) had come under Dr. Bosworth's observation. Five were in patients under the age of ten, forty-three between ten and twenty, twenty-three between twenty and thirty, two between thirty and forty, one between forty and fifty, and one in a patient over fifty. Of these, forty nine were females and twenty-six males. These cases all presented for treatment of nasal catarrh. We thus find the largest proportion of these cases occurring in patients below the age of twenty. The deduction from this is that the disease is essentially one of childhood, and that with the development which occurs at puberty, these growths gradually disappear.

The existence of these growths has long been recognized, but was first accurately described by Luschka, from whom the disease received the name of Luschka's tonsil. Woakes, of London, describes adenoids as consisting of a papillomatous structure; Meyer, of Copenhagen, and Loewenberg, of Paris, described them as being glandular. The analogy between these and the faucial tonsil seems complete, both in an anatomical, physiological, pathological, and clinical point of view.

Symptoms.—The most prominent symptom to which these growths give rise is that of an abnormal secretion, which is composed of mucus or muco-pus. The morbid process giving rise to the hypertrophy does not destroy the secreting function of the glands, but rather increases it. The secretion makes its way out through the nostrils or down the throat.

Another symptom is a change in the voice. Their presence gives rise to what Meyer calls the dead voice, the nasal resonance being destroyed as in an ordinary cold, so that *M* and *N* become *Eb* and *Ed*. The fact of even a small adenoid growth in the vault of the

pharynx upon a singing voice offers many points of interest. The conversational voice does not tax the full capacity of the larynx. A person with a considerable degree of laryngeal disorder will experience no special difficulty in an ordinary conversational use of the voice. Singing, however, taxes the very highest capacity of the larynx, and demands that not only the larynx, but the pharynx and the nasal passages shall be in a state of perfect health. In the lower or chest register a healthy larynx of course is essential, and notes in this register are taken with ease and clearness even when disease may exist in the nasal cavity. The vocal cords regulate the pitch tension of the voice, but the quality of the voice is governed or depends upon the nasal cavity, largely; so that while it is a matter of common observation among singers that it is the head register that first gives out, this illustrates what Dr. Bosworth has always urged, that the so-called catarrhal laryngitis is, in the majority of instances, a secondary affection and depending on the catarrhal disease of the nasal cavity. The laryngitis therefore becomes merely a symptom of the nasal disease. The method by which the singing voice becomes affected from the presence of adenoid growths in the vault of the pharynx is quite simple. The prominent function of the pharynx is that of a sounding-board, and in order that this sounding-board function shall be exercised, the pharynx must necessarily present a smooth, rounded, dome-like cavity against which the vocal waves may strike and be reflected. If, then, this region is the seat of an adenoid growth, the voice becomes muffled, and loses something of its proper resonance and quality. The singer becomes conscious of this loss of tone and unconsciously, perhaps, makes a renewed effort to overcome this obstacle. In doing so he ruptures some of the capillaries in the larynx or muscular fibres, thus bringing on secondarily a laryngitis. A strained voice is the result.

Impairment of hearing is not an infrequent accompaniment of adenoid disease in the vault of the pharynx. It is a common belief that this symptom is due to an extension of catarrhal disease into the Eustachian tubes. Dr. Bosworth was disposed to regard this as a very infrequent occurrence. In making an examination by inspection of the orifices of the Eustachian tubes, it is exceedingly rare to find any evidence of catarrhal inflammation in them.

Dr. Bosworth also said that it was commonly believed that the ear symptoms were due to pressure upon the Eustachian orifices. Adenoid tumors are soft, pulpy masses, having very little resistance, and, as the Eustachian orifices are composed of hard, dense, cartilaginous, or horny tissue, they could not likely be affected by pressure of soft adenoids. The real explanation of impairment of hearing, urged by Dr. Bosworth, was that it was due to interference with the renewal of air in the middle ear. The constant and ceaseless impulse of vocal waves upon the tympanum necessarily acts to rarefy the air in the middle chamber. To counter-balance this, we have the movement of the faucial muscles. The presence of an adenoid growth very markedly interferes with the free play of the levator palati muscles, and, therefore, interferes with the renewal of air function. The growth, of course, does not oppose any obstacle to the levator palati muscle itself, but the elevation of the palate. Moreover, any cause which interferes with free nasal respiration, acts to produce a certain amount of rarefaction of air in the pharyngeal vault. It is thus that nasal stenosis, to which the presence of an adenoid growth gives rise, acts still further to rarefy the air in the middle chamber, thus causing impairment of hearing.

Recurring attacks of earache were present in a certain number of cases, and in two cases there were attacks of otitis medii. In one case, tinnitus aurium existed, with impairment of hearing, which was markedly relieved, though not cured, by the removal of the growth.

Nasal stenosis is a prominent symptom in the presence of an adenoid tumor. It is due to the mechanical interference with the to-and-fro flow of currents of air. It is a very noticeable fact in these cases that under the influence of damp weather these growths become swollen and turgid to such a degree that they are very markedly larger on inspection at one time than at another. The point examined will sometimes appear very small; at other times puffed up. In this feature, the disease presents some of the characteristics of nasal polypi.

The disease is purely a local one, the inflammatory affection involving these glands always manifesting itself in a hypertrophy, and never in an atrophy. The morbid process confines itself to these glands.

In four of Dr. Bosworth's cases, disease occurred in connection with atrophic rhinitis, but in by far the larger proportion of cases it occurred in connection with rhinitis hypertrophica. In about one-half of the cases it occurred independently of any other morbid process.

Diagnosis.—The diagnosis is made by means of a rhinoscopic examination. Loewenberg and Meyer direct that digital exploration should be made in all cases. This, Dr. Bosworth did not consider to be necessary. It was only accomplished with discomfort to the patient, and unnecessarily excites more or less terror and apprehension on the part of the child. A better and clearer conception of the size and position of the tumor can be obtained by means of the rhinoscope. To make an examination, sunlight should be used in all cases, as the ordinary gas jet is not sufficient to thoroughly illuminate the parts. That there is any difficulty in making a rhinoscopic examination of children is an entire mistake. The confidence of the child is easily gained, and with a little adeptness of manipulation and patience, a satisfactory inspection can be obtained. Dr. Bosworth recalled no single instance where he had not been able to make this examination in children.

Treatment.—The treatment consists in the removal of the growths. The question arises whether an operation is demanded, in view of the fact that they sometimes disappear with the development that occurs at puberty. Dr. Bosworth claimed that their presence always gave rise to symptoms sufficiently prominent to demand their removal without regard to this feature. Not only should the existing symptoms be relieved before removal, but the threatened dangers of disorder lower down as a result of their presence. The presence of this growth undoubtedly gave rise to a tendency to weak throats, and bronchial troubles as the result of the habitual mouth breathing which they induce.

Various devices have been used for extirpating these tumors. Meyer, of Copenhagen, uses a ring knife. This consists simply of a ring with the cutting edge on the inside, mounted on a straight rod. The instrument is passed through the nose, and manipulated by means of the finger passed in the mouth behind the palate, and the growth shaved off. He follows this operation by passing a long curved instrument, having a rasp on its convex surface, by means of which instrument passed into the mouth he rasps the base of the tumor. Finally, he cauterizes the surface. Meyer unquestionably does his work very thoroughly as his record shows. He has operated upon between three and four hundred cases.

The question arises, cannot this work be done by much simpler means? Woakes, of London, Cohen, of Philadelphia, and others recommend a pair of curved

forceps with a cutting bite, which is passed behind the palate, and the growth seized and torn away. This seems to be a rude and harsh procedure. Loewenberg, of Paris, Michel, and Voltalini use the galvano-cautery. There are two methods by which this device may be used: Either by the heated wire severing the mass, or by destroying it by the flat electrode. The first method is not feasible. As a directly destructive agent, Dr. Bosworth believes the galvano-cautery to be immensely over-estimated. It is an expensive and ponderous instrument, and always getting out of order; and, moreover, for the purposes of destruction it does not accomplish what has been claimed for it. If a cold electrode is placed upon the growth, and the current closed the loss of heat is so great that it is impossible to develop even a dull red heat. The destruction of tissue is therefore limited. If heat be developed in the electrode before entering, it is almost impossible to pass it without burning healthy tissue. MacKenzie recommends the use of the curette. This instrument Dr. Bosworth formerly used, but found it difficult to remove any but simple growths with it.

The instrument which the author of the paper made use of was an adaptation of Jarvis's snare. In his hands it had answered a most admirable purpose in the removal of these growths. The distal end of the tube is bent to a quadrant of a circle, with the radius of one and a quarter inches. The instrument is fitted with a loop of No. 5 piano wire, of a size which it is judged will embrace the growth. The loop is now bent forward to a right angle with the distal orifice of the instrument. The wire is then drawn out about one-eighth of an inch, and the whole of the loop is now bent backward toward the hand. The wire now has two kinks in it. The instrument with the loop in this position is readily passed behind the palate and to the vault of the pharynx with the extremity of the tube lying immediately behind the tumor. As the screen at the proximal end of the instrument is slowly turned, the loop is made to swing up and embrace the growth, which is slowly severed. The rigidity and firmness of the steel wire render this procedure an exceedingly simple matter. By this simple device even a broad sessile growth can readily be removed. The operation is attended with but little pain and trifling hemorrhage, and is not difficult to perform. The growth is either drawn out with the instrument or blown into a handkerchief through the nose. Dr. Bosworth has never had the slightest inconvenience from these growths dropping into the air passages. He has, however, occasionally lost a specimen by its being swallowed. No anæsthetic is required. The operation of course necessitates a certain amount of faucial control on the part of the patient, as the loop cannot be passed unless the palate is entirely relaxed. Occasionally, Dr. Bosworth has been compelled to operate through the nose in young children on account of an irritable throat. In these cases an anæsthetic has been given, and the operation performed with the ordinary straight Jarvis's snare. The method by which this has been done is as follows: The snare is fitted with steel wire as before, the loop being of proper size to embrace the growth. The loop is then bent downward, and to a right angle with the orifice of the tube. Having done this, it is drawn into the tube until sufficiently small to pass readily through the nares. It is then passed in a vertical position until it reaches the pharynx, where it is turned in such a manner that when the wire is pulled out through the tube, the kink which has been given it, will turn the loop downward. When in this position the end of the instrument is pressed firmly against the pharynx, and the loop drawn home by means of the screw at the proximal end of the tube. The same procedure is repeated in each nostril. Complete anæsthesia is not necessary, as the operation is completed in from one to two minutes, the child being immediately aroused and directed to blow its nose.

Nasal Polypus.—The next class of nasal tumors to which Dr. Bosworth called attention was nasal polypi. This disease has been recognized since the days of Hippocrates, and is treated of in all text-books to the present day. The morbid condition consists of the presence in one or both of the nasal cavities of one or more soft gelatinous bodies which block up the passages, and give rise to more or less profuse secretion or discharge.

Properly speaking, the term polypus means pedunculated tumor. As a clinical fact, the only pedunculated tumors met with in the nose are nasal polypi. In structure these tumors are pure myxomata. A proper classification would demand that these tumors be recognized as such. Fibromata never become pedunculated. They are always sessile growths. In current literature a fibroma of the nasal pharynx is usually spoken of as a naso-pharyngeal polypus. During the last year there were treated at Dr. Bosworth's clinic at Bellevue Hospital 1641 cases of throat and nose affections. Of these, there were 19 cases of nasal polypus. During the same time he saw and operated upon 16 cases in private practice. Of these 35 cases, 2 were under 20 years of age, namely, 11 and 15 years; between 20 and 30 years of age there were 9 cases; between 40 and 50, 12 cases; between 50 and 60, 6; between 60 and 70, 4 cases; 20 were males, 15 females. In a large number of cases the disease occurs in both nostrils. In those cases in which they are found only in one nostril the tumors are few in number and small in size. This fact would argue that the disease commences in one nostril and subsequently develops in the other. They spring from the under surface of the middle turbinated bone in a large proportion of the cases. Dr. Bosworth recalled but two cases in which they sprang from the septum. They are pear-shaped bodies attached by a narrow pedicle. Their shape is probably due to their soft consistency. Under the influence of gravity and the to-and-fro movements of inspired air they are drawn out and hang in the cavity. They rarely assume much size, simply from the fact that they have no room for development in the narrow nasal passages. Occasionally they develop near the posterior nares and drop into the pharynx, where they have more abundant room for growth, and therefore attain a larger size. Dr. Bosworth showed two polypi nearly of the size of hens' eggs, which were removed by operating through the nares. We were generally taught that this disease was the result of nasal catarrh. Dr. Bosworth thought that nasal polypus is a disease by itself, having no special connection with nasal catarrh or chronic rhinitis. In a majority of his cases, after the removal of the polypi, the nasal mucous membrane was found healthy. In a certain proportion of cases there was chronic hypertrophic rhinitis. In four cases there was atrophic rhinitis, which would seem to argue that there is no connection between these affections and hypertrophic catarrh. In two instances the writer had removed polypi from the borders of syphilitic ulcers in the nose. This would indicate no connection between syphilis and nasal polypi, the growths being simply the result of local morbid changes.

Ordinary symptoms were briefly referred to. They consist in a watery discharge, the hydroscopic character of the tumors, a nasal stenosis, etc. Sneezing was referred to as a prominent symptom of nasal polypus, and one whose importance is usually underestimated. The irritability of the nasal mucous membrane in chronic rhinitis is impaired, and sneezing is not a symptom of ordinary chronic nasal catarrh. In

the early stages of nasal polypus, repeated and violent attacks of sneezing are very common. This symptom is always suggestive of the disease, and when it exists without apparent cause, the nasal cavity should be explored.

In three cases asthma was present, and depended upon the existence of these growths. The asthma presented all the characteristics of ordinary spasmodic asthma, the physical signs, together with nocturnal exacerbations, being present. In two cases the asthma was entirely cured by the removal of the polypi without the administration of remedies. In one case in which it had existed for two years, the asthma was only relieved by the administration of iodide of potassium for three months after the polypi were removed. This reflex symptom of nasal polypus (asthma) was first noticed by Voltalini, in 1871. Since then additional testimony has been adduced by Daly, Frankenkel, Rumbold, Spencer, Todd, Porter, and Holden.

Treatment.—The earliest treatment recommended for the removal of these growths was that by Hippocrates. This consisted in passing a string through the nares, to the end of which a large piece of sponge was tied, which was then drawn back through the nasal passage, bringing out with it some of the growths. It is a strange fact that this procedure is still recommended in some books.

Injections with acetic acid, tincture of iodine, persulphate of iron, and corrosive sublimate have been recommended. The result of the use of these injections into the growth is its necrosis. The polyp sloughs away after from three to five days, during which time the patient suffers from offensive, watery, and fetid discharge from the head. It is impossible to inject more than one or two polypi at a time. Hence, for the eradication of all the growths, a long course of treatment is necessary, during which time the patient is practically suffering from an offensive form of ozæna. All works on general surgery recommend the use of forceps for the removal of these tumors. This method is perhaps the one most universally resorted to. We are directed to pass the forceps in and seize the pedicle of the polyp, and drag it out. Dr. Bosworth said that he had never seen a pedicle of the polypus until after it was removed. Hence this procedure is impossible by inspection. If it is meant to pass the forceps in and feel for the pedicle, the result is that a harsh instrument is passed into an exceedingly sensitive cavity, and made to grope about, until something is seized upon, whether it be healthy or diseased tissue, and is dragged forth. Healthy parts, in all cases, are bruised and mutilated. Violent hemorrhage is the result of the first introduction of the forceps, and all subsequent procedures are entirely in the dark. It is one of the most unsurgical proceedings that Dr. Bosworth knew of. Moreover, all the tumors cannot be removed by the forceps. We are, therefore, directed to remove a portion of the turbinated bone. The healthy mucous membrane upon the turbinated bone has a certain function to perform in the economy, and its removal is unjustifiable without sufficient cause.

The galvano-cautery recommended by Middledorphf is probably efficient. Dr. Bosworth's objection to this was that it was a bungling instrument. Moreover, the use of a heated wire in the nasal cavity is liable to do a great deal of injury to healthy tissue. The introduction, by Hilton, of the snare was the first thoroughly surgical method of removing these growths. The snare was mounted with the soft wire, which was passed around the growth, tightened, and the polyp evulsed. This procedure was liable to leave a portion of the polyp tissue in the nose.

Jarvis's snare écraseur furnishes us with an instrument which leaves nothing to be desired. The steel-wire loop with which it is provided being firm and resisting, can be carried in any direction in the nasal cavity, and easily manipulated so as to embrace the smallest growth. This difference should be noted between the action of the snare and the action of the écraseur. In the snare the growth is seized and evulsed, in the écraseur the growth is embraced and severed by a slow process of écrasement. With this instrument, when the operation is done deftly, a long shred of fibrous tissue is easily dragged out from the healthy mucous membrane. Dr. Bosworth was not a believer in roots, but if there was a root of a polypus this is the root, and another polypus does not develop at this spot. In the specimen shown, this fibrous shred was easily recognizable. The only objection to the ordinary snare is that in deeply seated growths the hand of the operator obscures inspection of the nasal cavity. To overcome this, Dr. Bosworth made a combination of Jarvis's écraseur and wire snare.

As to the recurrence of these growths Dr. Bosworth expressed the opinion, that if they were thoroughly relieved they would not occur. The operation, it should be stated, is done with but little pain and no hemorrhage, and can be continued from day to day until all the growths are removed. He had not found it necessary to apply a cautery to the base in order to prevent the recurrence of the growths. They were to be treated as you would weed a garden.

DR. LEFFERTS was called upon to open the discussion. In doing so, he said that there were many points in Dr. Bosworth's excellent paper that needed no discussion. On the other hand, there were some statements made by Dr. Bosworth which he thought it would be well to consider. First among these was that adenoid tumors of the naso-pharynx were generally believed by the profession to be rare. Dr. Lefferts declared that any one who had read the journals could not help having noticed this subject treated of almost *ad nauseam*. The speaker, however, had not found these adenoid vegetations so comparatively common as the statements of Dr. Bosworth and the numerous pathological specimens presented by him this evening would seem to indicate.

The fact that a large number of these growths were exceedingly small and could hardly be considered as an abnormality, should not be overlooked. The author of the paper had not laid sufficient stress upon this point. The question was asked, is it always necessary to remove these growths? Dr. Lefferts believed not. His reasons were that at a certain age, namely, that of puberty, these growths disappear by a gradual process of atrophy. Hence, he thought it was desirable not to operate in any of these cases unless the symptoms were of an aggravated character. He agreed that there were cases causing a certain amount of interference with a child's voice, and giving rise to other disagreeable symptoms which required attention. The majority of children that were brought to him having these growths, were brought on account of having nasal catarrh. There was usually found a condition of hyper-secretion. In these cases it was his practice not to operate, but to wait until the time had passed for the atrophy of these tissues to take place. Hence, he would not advocate their treatment by removal.

As to treatment, Dr. Bosworth had run over the various methods in use, and summed up by recommending the method of removal by means of Jarvis's snare, which instrument, the speaker said, had figured largely in the author's writings of late. This instrument answers the purposes of removal very well, but it has not met with general approval on the part of the profession. The speaker stated that it was not generally used by those engaged in this special practice. While in Europe, Dr. Leffert's attention had been called

to a new form of forceps which answered every purpose, and did not require for their use so much skill as Dr. Jarvis's snare. These forceps had been used by him extensively, and with gratifying results.

Two forms of nasal tumor, not mentioned by the author of the paper, were alluded to. First, that of cystic tumors of the nasal passages. This was a rare affection, but was worthy of notice, being of diagnostic importance. The speaker had met with but one case, and knew of but one in literature. Cystic tumors resembled exactly in their appearance gelatinous polypi. They differ from the latter in that in operating after seizing with the forceps, they are ruptured, and their contents escape. Under these circumstances, ordinary operators would continue to grasp for growths which did not exist, and hence might do harm. Dr. Lefferts stated that he himself had committed this error. The second form of nasal tumor sufficiently common to justify its mention was that of papilloma. These grew far forward. In several cases the speaker had seen them give rise to persistent attacks of epistaxis. It was a simple matter to put such a patient in a suitable light, and make an examination of the growth, upon detecting which, it could be readily removed by means of the forceps or scissors. He did not agree with Dr. Bosworth's statement, that interference with the singing voice was usually first noticed in the upper register. His experience had been that the middle register was the first to be affected. This, however, was a subject the discussion of which the limits of his remarks would not permit him to enter into.

DR. BRANDEIS wished to take issue with the author's statement regarding the use of the galvano-cautery. He had used it for a number of years past, and had thus far not met with any of the evil consequences described by authors. He had never had any affections of the middle ear resulting from its use, nor sufficiently extensive cicatrices to cause malposition of normal parts. The offensive discharge which was incidental to this method of operating was of only temporary duration, and of trivial annoyance. He had removed tumors by this means through both the nasal and pharyngeal cavities. The removal of growths by means of the galvano-caustic loop was considered good practice for two reasons: it not only removed the growth at its base, but, by virtue of its intense heat, reduced the probability of a recurrence of the trouble to a minimum.

DR. LINCOLN thought there were many growths in the nasal passages, especially anteriorly, the evil results of which might be far-reaching. In regard to those in the posterior region, the necessity for operation depended entirely upon whether the consequences of their continuance were sufficiently great to impair the health or produce great discomfort. The discomfort would depend upon the size of the tumor, and the discharge in consequence thereof. A case of reflex asthenia, due to one of these small growths, was cited. The growth was so insignificant that its removal was only finally resorted to as a matter of experiment, and the result which followed its abolition was an entire relief from asthmatic symptoms.

Another evil effect which might arise from the presence of these growths was that of malformation of the chest produced in childhood. The speaker had seen a number of instances where no other explanation by which the production of this deformity could be accounted for than that it was due to faulty respiration, incidental to the presence of growths in the nasal passages. Children thus afflicted would have short breathing, and breathe with their mouths open, causing a sucking movement, and consequently an insufficient expansion of the chest.

He believed that the author of the paper intended to infer that no method other than the use of the snare afforded satisfactory results. Upon this point he must take issue with Dr. Bosworth. He had published a series of cases some years since, in which caustics were used, and where the results were eminently satisfactory. The Vienna paste was a substance which could be applied by any one, and was most certainly a successful method of removing growths from the nasal passages. He had often applied it to the tonsils in cases of their enlargement, and with satisfactory results, reducing the tonsils to their normal size in a short time.

Jarvis's snare was certainly a most excellent and serviceable instrument. For certain cases, he believed that there was nothing else that could be devised to take its place. It was the most useful instrument in his office. He, however, recognized that in many cases there were other means of obtaining satisfactory results. The tonsils, as a rule, become atrophied at puberty. In cases of enlarged tonsils, he applied boracic acid, and with the result of diminishing the size of the growth.

DR. ROBINSON remarked that most of the points which had occurred to him during the reading of the paper had been touched upon by those who had preceded him in its discussion. In regard to Jarvis's snare and the warm advocacy of its use by Dr. Bosworth, the fact remained that it was not the only instrument we could use for the removal of these growths. It was, however, undoubtedly the best instrument which could be selected for that purpose when the growths were of any considerable size. Of course, when they were small, flat, and sessile—and such cases were very numerous—they could be removed by the galvano-cautery, or scarification with Myer's knife, or a sharp curette. But for the large growth, as he had already stated, there was no method that could at all compare with Jarvis's. In two cases, where other methods had failed entirely, he had used the snare and readily removed the growth without great pain or much trouble.

There was one point which he thought was not sufficiently emphasized by the author of the paper, and that was that when these adenoid growths were of considerable size, a certain amount of air was sucked out of the Eustachian tubes during each act of deglutition. He believed that the extension of the inflammatory action up the tubes was the explanation of imperfect hearing following the development of these growths. It was a curious fact, however, that in some instances the hearing was notably affected, while in others it remained good, notwithstanding the existence of growths of considerable size.

DR. JARVIS being called upon by the Chair, it was learned that he was absent in consequence of sickness.

DR. BOSWORTH, in closing the discussion, commented upon the question as to whether it was desirable to operate for the removal of these growths in children under the age of puberty. He stated that these children were brought to us in a morbid condition. They were brought on account of the existence of permanent and annoying symptoms, from which they wished to be relieved. It seemed to him that it was our duty to relieve these symptoms if we could. There was an additional reason for such a practice, as he had already stated. These growths naturally have a tendency to involve the parts below, giving rise to throat and bronchial troubles. Furthermore, we were not at all sure, that they would disappear upon the patient reaching the age of puberty, inasmuch as one of the pathological specimens presented this evening was from a patient sixty years of age.

As to the question of the forceps or the snare, he would say that if one or two of the growths which he had presented this evening were carefully inspected, it

would be seen that they bear the marks of the forceps. In fact, one of the growths which he presented had been chopped at with the forceps for three months by an expert, and had afterwards been removed by him by means of the snare. He had never seen a pair of forceps that could be made to reach the whole of one of these adenoid tumors. As to extension of catarrhal disease through the Eustachian tube, he could say that he had very rarely seen any evidence of catarrhal inflammation at the Eustachian orifices when inspecting them. His explanation of the interference of hearing due to the presence of these growths was not that they pressed upon the orifice of the Eustachian tubes and thus occluded them, but that, when of considerable size, they interfered with the action of the levator palati muscles which were concerned with the entrance of air into the middle ear.

NEW YORK NEUROLOGICAL SOCIETY.

Stated Meeting, January 2, 1883.

THE PRESIDENT, E. C. SPITZKA, M.D., IN THE CHAIR.

PRESENTATION OF A SPECIMEN OF OCCLUSION OF BASILAR ARTERY.

DR. LEONARD WEBER first read the history of a patient who had died in consequence of occlusion of the basilar artery, which was, in brief, as follows:

The patient, a male, 42 years of age, and a merchant by occupation, had been, for a few weeks previous to his visit to Dr. Weber, feeling unnecessarily tired and weak. His appetite was diminished, he had occasional nausea, irregular action of the bowels, and frequent headaches of the nature of painful pressure on the top of the head; also, disturbed sleep. He came under observation November 9, 1878. On Nov. 21 he had not improved. He complained of vertigo and of a constant roaring noise in both his ears, and of increased pressure on the top of his head. In walking, he now dragged the right leg a little. The patient looking quite ill, a carriage was ordered for him, and he was taken home. Shortly afterward, he was found with paresis of the right upper and lower extremities, also right half of face and tongue. Eyesight was normal, as far as ordinary tests were concerned. He complained of vertigo, sickness, and increased pressure on the top of the head; no pain elsewhere. The patient gave a history of having had a primary sore on his penis twelve years previous, which, however, was not indurated, and was not followed by any secondary symptoms. He also gave a history of having subsequently had an ulcer upon his heel, which came without apparent cause, and remained open for a long time. Finally, under the influence of iodide of potassium, this ulcer healed. From these facts, it was supposed that his trouble now was probably due to syphilis, there being no other evidence of other organs being diseased which might cause embolism or cerebral apoplexy. At 2 P.M., Nov. 22, the patient was taken with a terrible fit of general convulsions, lasting several minutes. After the first attack the patient lost consciousness. These attacks were repeated every half hour, and the patient died in deep coma at 10.30 A.M.

A post-mortem examination twenty-four hours afterwards revealed no evidence of disease of the larger arterial branches until coming in contact with the basilar artery. This vessel was found completely filled with a firm clot extending from the place of union of the two vertebrals, about half an inch upwards. Upon making a longitudinal section, a small dense tumor growing from the inside of the walls of the right vertebral was cut through just at the junction with its fellow, and almost completely obstructing the lumen of the basilar artery at its very commencement. A microscopic examination revealed the fact that the tumor was composed of small cells and connective tissue, corresponding in its character to similar gummatous affections of cerebral vessels as described by several authors.

DR. WILLIAM A. HAMMOND then read a paper on

ALLOCHIRIA, ITS NATURE AND SEAT.

The case reported was one which Dr. Hammond saw in November in conjunction with Dr. L. A. Stimson. It was asserted that the patient had received a serious injury to the spine. In a day or two after the accident, symptoms indicating spinal trouble began to develop. Some time afterwards a physician was consulted, who diagnosticated Pott's disease. A plaster jacket was applied. Several months elapsed, during which he was at times better, and at other times worse. Upon the whole, there was no decided improvement. The fact that he had brought an action for heavy damages against the city was the immediate cause of Dr. Hammond's examination. Dr. Hammond was of the opinion that the patient had never suffered from an injury of the vertebral column or of subsequent Pott's disease. This opinion was concurred in by Drs. L. A. Stimson, Hamilton, and Clymer. The patient complained however of pain throughout the whole spine, and of excessive nervous irritability. He had had contractions of the muscles of the lower extremities, and on causing him to walk about the room, it was evident that his limbs were stiff and that he lifted his feet with difficulty. His gait was not that of locomotor ataxia. Up to this time, no experiments had been made with a view of testing the sensibility of the lower extremities. These were now denuded of their clothing, and the patient was told to shut his eyes. The touch of a finger, the scratch of a pin, or a deep puncture with the blade of a penknife, was equally unfelt in the right leg. On making the like experiments on the left leg, he complained of pain when the knife was stuck into it and automatically carried his hand to the place where he supposed the puncture had been made, but instead of touching the spot injured, he indicated the exactly corresponding situation on the other leg. Repeated experiments led to like results. The conclusion was reached, that the patient was suffering from antero-lateral or lateral sclerosis, with the implication of the posterior horns of gray matter, and probably of the membranes of the cord to a slight extent.

It was with reference to this cross sensibility which the patient exhibited that Dr. Hammond's paper was concerned. To this condition the name of allochiria has been given by Prof. Obersteiner, of Vienna, who was the first, so far as Dr. Hammond knew, to call especial attention to the phenomenon, though it had been incidentally alluded to by Leyden and Ferrier. In a post-mortem examination of one of Obersteiner's cases it was found that there had been inflammation of the first, second, and third lumbar vertebræ, meningitis, and extensive transverse inflammation of the cord. The posterior columns for a considerable distance above the seat of the injury were in a state of sclerosis, and the posterior horns of gray matter in portions of the cervical enlargement were transversely divided by a peculiar, structural, transparent mass intensely colored by carmine, and very similar to the mass which is found around the larger vessels in inflammatory processes in the cord.

Dr. Hammond was of the opinion that the lesion above described of the posterior horns of gray matter was that to which the phenomenon of allocheria is to be ascribed. Obersteiner and Ferrier had declared their inability to give an explanation of the mechanism of

the production of allochiria. Preparatory to an explanation of this symptom, Dr. Hammond referred to the fact that the posterior tract of gray matter was probably the only channel by which sensory impressions reached the brain, the posterior columns having in their normal condition nothing whatever to do with the transmission of such impressions. But, before reaching the posterior horns, the posterior roots of the spinal nerves pass through the columns of Burdach, and when these are the seat of inflammation, as they are in locomotor ataxia, disturbances of sensibility, such as hyperæsthesia, paræsthesia, and anæsthesia are produced in the parts below by the pressure exerted upon the roots. It was quite certain that there is an almost complete decussation of the sensory fibres within the gray matter. We were taught these facts not only by experimental physiology, but also by the instruction we derive from the study of cases of disease or injury of the cord. A diagrammatic representation of a section of the cord was then displayed and an explanation given of the method by which the peculiar symptoms alluded to arose. Dr. Hammond stated that in those cases of disease or injury of the posterior horns of gray matter, whether they be primarily involved or secondarily, as in locomotor ataxia, in which allochiria exists, either the lesion must be unilateral, or, if both horns are involved, the lesions must be at different levels.

In the only case of allochiria in which a post-mortem examination has been made, and which has been referred to above, Obersteiner found, among other abnormal conditions, disease of both posterior horns of gray matter. The morbid process was not continuous, as it is stated that it was not perceived in all the sections. It was situated at the narrowest part of the posterior horns, being so placed as to interrupt the decussation of all the nerve fibres, and hence to cause a transmission of sensory impressions upward on the same side in which they entered—a condition which, equally with that which Dr. Hammond described, would give rise to allochiria.

PATHOLOGICAL SOCIETY OF PHILADELPHIA.

Stated Meeting, Thursday Evening, Dec. 14, 1882.

THE PRESIDENT, DR. JAMES TYSON, IN THE CHAIR.

A CASE OF MITRAL OBSTRUCTION, WITH SEQUENTIAL LESIONS.

DR. E. T. BRUEN presented this specimen, in which the auriculo-ventricular opening of the left heart is nearly occluded by an epiglottic-shaped enlargement of one of the leaflets of the mitral valve. The valve is very much thickened, and the focus of a considerable calcareous deposit. The orifice during life permitted a reflux of blood from the ventricle into the auricle. The left auricle is dilated and hypertrophied, so that its cavity is about twice as large as normal. The right ventricle is very much dilated, the walls of this cavity are less than half the normal thickness, and the ventricle must have had during life twice its physiological capacity. The tricuspid valves were insufficient. The considerable cardiac enlargement, occasioned an increase of the area of dulness on the level of the third and fourth ribs, and to the right of the median line of the sternum. In children, the heart with similar enlargement encroaches upon the left pleural cavity to such an extent that the physiological inflation of the left lung cannot occur. Bronchial breathing is produced, audible posteriorly, while anteriorly, below the second interspace, no respiratory murmur is audible. In these cases when the complication of bronchitis occurs, the physical signs suggest a pleural effusion. Enlargement of the right ventricle, both in children and adults, causes a pronounced impulse at the epigastrium and occasions serious pain and inconvenience. The murmur heard during life, in the case from which this specimen was taken, indicated this lesion, both presystolic and systolic murmurs being audible. The second sound at the pulmonary artery cartilage was also much accentuated, owing to the repletion of that vessel with blood. The first sound over the right ventricle was very clear and distinct, as is common in these cases, but the first sound at the apex was obscured by the murmur. The patient from whose body these specimens were removed was a woman, aged 46 years, who had been subject to heart disease since 20. The immediate cause of death was pulmonary repletion with blood, which induced right-heart failure.

PERICARDIAL EFFUSION AND ADHESION OF THE PERICARDIUM TO THE APEX OF THE HEART MISTAKEN FOR HEART RUPTURE.

DR. J. T. ESKRIDGE presented the specimens, and said that in this case the physician making the autopsy actually considered the specimen to be one of heart rupture. The patient was an athletic young man, and had been perfectly well until a few days before he sought medical advice. He was under treatment for only twenty-four hours, suffering from cardiac pain and great prostration. He died suddenly and unexpectedly, when no one was near him. The attending physician, who made the post-mortem examination with no professional assistance, reported effusion in both pleural cavities, the pericardium distended with thin, non-coagulated blood, and a rupture of the left ventricle. Dr. Eskridge said that a careful examination of the heart, pericardium, adjacent glands, and portions of the larger bronchi, showed marked evidences of pericarditis and pleuro-pericarditis. The pericardium was adherent to the lower third of the heart, but the adhesions were recent and easily severed. The heart was not much enlarged, its valves were nearly normal, and its muscle firm. No rupture was found. He believed that the case was one of pleurisy and pericarditis with effusion, death taking place suddenly from mechanical interference with the heart and lungs. He thought that the most plausible explanation of the doctor's mistake in calling it a case of cardiac rupture was that, when severing the blood-vessels around the heart, blood flowed into the pericardium and mingled with the serous effusion. He did not think that a firm, non-fatty heart could rupture itself by its own contractions. If the pericardium was filled with effusion in that instance, it taught a lesson of far more practical value than a case of cardiac rupture under similar circumstances would. It was evident, if the pericardium should be attached to the apex of the heart in a case of pericardial effusion in which operative interference was determined upon to free the heart's action, a thrust of the trocar into the pericardium would greatly endanger the ventricular walls.

SEVERAL SPECIMENS OF EYES ENUCLEATED ON ACCOUNT OF SYMPATHETIC IRRITATION IN THE OTHER EYE, OR FROM FEAR OF ITS DEVELOPING,

were exhibited by DR. LITTLE, who said that sympathetic irritation and sympathetic ophthalmia are the only two forms of the sympathetic diseases of the eye that afford an opportunity for pathological study, and in these cases only the enucleated eye can be investigated. Just what are the conditions in the remaining eye must remain uninvestigated. If full restoration of the function of vision is attained, great satisfaction only is felt. Less and less opportunity is being afforded of studying the condition of an eye enucleated for sympathetic ophthalmia in the other eye, since

merely the sympathetic irritation of the sound eye impels the surgeon to enucleate the primarily affected eye before true sympathetic ophthalmia asserts itself. The portion of the eyeball which renders liable the development of this affection when diseased is so well known that not even sympathetic irritation should be allowed to develop, as an early enucleation will prevent it. Enucleation of the primarily diseased eye when true sympathetic ophthalmia is present in the other eye is now questionable, as after all inflammation has subsided under treatment, surgical procedures upon the primarily affected eye may afford the best results for visual purposes. A recent experience will bear out this statement, the patient refusing the advice of a former medical attendant, and also my own, after two months suddenly developed sympathetic ophthalmia of the sound eye. Enucleation was then too late; now both eyes are becoming quiet, and I am in doubt which in the end will be the more available eye. A physician recently under treatment for a severely traumatized eye has refused the advice of two surgeons and is now doing well, but the danger of a sympathetically irritated eye is constantly before him. The pathological investigation is, then, mainly restricted to eyes enucleated before or after sympathetic irritation has developed in the other eye, as results show that under such circumstances full protection to the remaining eye is afforded. Investigation of eyes enucleated when sympathetic ophthalmia of the other eye is present may explain the cause of the trouble in the remaining eye, but there is so much damage done to both that our knowledge only makes us the more desirous to prevent these conditions from arising, and in the multiplicity of conditions the principal cause is lost. When enucleation to forestall sympathetic irritation is done, or when the operation is performed with sympathetic irritation just beginning or present, the eyeball removed is in a much better state to examine, and more light can be thrown upon the cause of sympathetic irritation, since severe inflammatory processes cloud the change from sympathetic irritation to that of sympathetic ophthalmia, and the pathological study is more difficult. My collection contains only one specimen enucleated when sympathetic ophthalmia was present, and in this case there was a double acute glaucoma, with sympathetic iritis.

At this time I only desire to place before you some specimens of eyes enucleated for the protection of the fellow eye from sympathetic irritation, or in which it was already beginning or developed; and in these cases good and permanent results have been attained. Four of these cases were due to traumatism; the fifth was of an inflammatory character. All but one of the traumatic cases had the fellow eye affected, and the examination of the enucleated eye in the exceptional case justified the operation. In two cases the sound eye became affected shortly after the injury to the enucleated eye. In one case, no irritation until forty years had elapsed since the accident to the enucleated eye. In one non-traumatic case, there were repeated attacks, for a series of years, of irritability in the sound eye, until the pain in the diseased eye, and the disturbance of the sound eye, compelled operation. In the remaining traumatic case, for twenty years the uninjured eye was unaffected, except rendering the myopia more progressive, which made an operation more imperative for its arrest, and to prevent the outbreak of sympathetic ophthalmia later in life. The patients were aged respectfully, three years, forty years, forty-seven years, fifty years, and seventy years. Three of the patients were males, and two females. In four cases the left eye was enucleated, in one case the right. In every case the injury or disease involved directly or indirectly the ciliary body; and where the crystalline lens remained *in situ*, or the sclerosed tissue infringed most markedly on the ciliary region, the irritation most rapidly in the other eye. Where the crystalline lens was dislocated with weakening of the sclerotic tissue, no irritation appeared for forty years in the other eye. In one case with dislocation of the lens, and detachment of the retina and choroid, no irritation had appeared at the end of twenty years in the other eye. In those cases where the iris became entangled in the cicatrix, sympathetic irritations of the other eye developed most rapidly, in one instance the lens becoming cataractous in the sound eye, while that of the injured one was either absorbed or lost at the time of the accident. In short, is it not to injury of the ciliary nerves, with their varied function, and to the damage done to the tissue in which they are imbedded in the different divisions of iris, ciliary body, and choroid, that we are to look as the cause of sympathetic irritation in the fellow eye; and of these, the ciliary body and nervous structures in it, with or without the involvement of the iris as the prineipal part involved? When sympathetic ophthalmia arises in the sound eye, can it not be traced to inflammatory processes added to the irritation of the ciliary nerves, and involving the iris, ciliary body, and choroid? How far the retina and optic nerve participate in the sympathetic irritation, it is difficult to say, only we find that in sympathetic enucleation for sympathetic irritation, the fellow eye regains the full function of sight. In sympathetic ophthalmia, however, the involvement of the nerves and retina is a more important factor, and the fellow eye is not so likely to be restored to full function of sight, as the conditions in the enucleated eye and in the one affected are more serious on account of marked inflammatory processes.

STAB OF PERICARDIUM, DIAPHRAGM, AND LIVER.

DR. STRITTMATTER exhibited three specimens, which were removed from a German laborer aged sixty-two, a patient in St. Mary's Hospital under Dr. Mears. He had been laboring for some days under a mental delusion, and after writing a clear, intelligible letter to his wife and family in Germany, stabbed himself with a clasp-knife in the right side of the chest several times. He was at once admitted to the hospital. He was much excited, with a rather flushed face, and but little shock. Examination showed two wounds—one over, and reaching down to the cartilage of the sixth rib of the right side, about one and a half inch from the sternal border, and about two inches in length; the other, half an inch from the border of the sternum, and severing from it the cartilage of the seventh and eighth ribs. Closer examination of this wound showed that there were two openings through the costal cartilages about a line apart, the outer passing downward, outward, and backward, the inner in a direction inward, downward, and backward. Through these openings air occasionally passed when the patient respired violently when struggling, causing a high-pitched sucking and blowing sound. None but slight bleeding took place, and that from the integument. Physical signs everywhere normal except over the lower part of the right lung, where increased resonance was noted, and on heavy percussion a kind of "cracked-pot sound" was elicited. Auscultation revealed diminished breathing sound over the upper part of the right lung, while respiratory sounds were entirely absent over its lower portion. Heart's action rapid and irregular; heart sounds feeble, especially the first; pulse, 112; respiration 42, and shallow. There was no sign of internal hemorrhage. Both during inspiration and expiration, when the head and shoulders were raised, and he inspired deeply, a peculiar high-pitched blowing sound was occasionally heard a little to the left and below the ensiform carti-

lage. It did not resemble the sound produced by gas in the stomach or bowels. Antiseptic dressings were used and morphia given, but he slept little, although nothing was complained of beyond a burning sensation beneath the sternum. The next morning he was calmer, rational, felt pain only during inspiration, and altogether breathed more easily than during the night, although the physical signs remained the same. The abdomen was tympanitic; the pulse full and moderately strong, 92 per minute; respirations 32, and expiration labored. During the next night grew worse. Friction sounds over the whole of the left chest, and on the right side from the apex to the fourth rib. Pericardial friction sounds were also heard. Pulse 100, full and hard; respiration 48, with rapid jerky inspiration, abruptly terminating, to be followed by a free, prolonged, groaning expiration; temperature, 103.2°. During the next twenty-four hours all the symptoms, physical as well as rational, of effusion into the pericardium and both pleuræ developed, but although the abdomen was tympanitic, no signs of effusion were detected. Pulse in evening 126, irregular and intermitting; respirations 40; temperature 101.8°. Had a bad night, and next morning appeared much prostrated, with a feeble, occasionally intermitting pulse of 120 per minute; temperature 102.8°; respiration 40, labored and shallow. Low, muttering delirium now set in; he sank rapidly, and died at 2 P.M. of the 20th, with a temperature of 105.5°.

Sectio cadaveris.—Brain: pia mater adherent, thickened in patches and opaque, especially on either side of the vessels, which were filled with dark blood over the upper convex surface of the left hemisphere. There was a slight amount of serous effusion in the subarachnoid space. The ventricles contained a small amount of serum. Chest: on raising the sternum the right pleural cavity was seen filled with a thick, fatty-looking effusion, with some bands of recent lymph, extending from the lung to the chest-wall. The apex of the lung was quite firmly adherent to the chest-wall. The anterior surface of the lung was covered to the depth of one-fourth of an inch with soft, grayish-yellow lymph. A portion of the back part of the inferior lobe was consolidated. The left pleural cavity was only about half filled with the same thick layer of lymph and adhesions of the apex that were noted in the right lung. No part of the left lung would sink in water. The pericardial sac was distended with fluid, and both upon its inner surface and upon that of the heart was abundance of lymph, connecting the two surfaces by drawn out bands of the same. There was a large chicken-fat clot in the left ventricle, extending about six inches into the aorta. The right ventricle was filled with blood, with a small clot extending into the pulmonary artery. Examination showed that, while there were but two penetrating wounds externally, the knife must have been thrust in repeatedly after partial withdrawal, as there were three openings through the diaphragm, penetrating the liver to the right of its suspensory ligament, and one traversing the lower part of the pericardial sac, and entering the left lobe of the liver for about one inch. The other liver wounds were one-fourth, one-half, and one-sixth of an inch deep. There were no traces of peritonitis, although about two ounces of serum were present, having probably escaped from the pericardial sac through the wound. The liver wounds did not gape; were united and surrounded for about an inch in every direction by a brownish-yellow discoloration. The liver weighed sixty-two ounces. Spleen enlarged and soft; other organs healthy.

DR. MEARS said he was much interested in this case. During life the symptoms of wound of the diaphragm and of the liver were markedly absent, whilst those of injury of pericardium and pleura developed as the interval after the receipt of the wounds increased. The external wounds gave little indication as to the direction taken by the knife after puncturing the thoracic cavity, and, as shown by the post-mortem examination, no information as to the extent of injury inflicted. The absence of symptoms of injury of the diaphragm may be explained by the fact that the wounds were in the tendinous portion of that muscle, and being small did not interfere to any great extent with its function in respiration. In injuries causing laceration of the muscular fibres attached to the ribs dyspnœa occurs as a prominent symptom, by reason of the impairment of the respiratory duty of the muscle. Moreover, the symptoms may have been masked by those referred to the injury of the pericardium, as in wounds of both of these structures dyspnœa is a prominent symptom. The knife in one of the thrusts passed through both, and involved them in a common injury. The only explanation I can offer of the production of the blowing or rather suction sound which was heard under the ensiform cartilage, is that it was occasioned by the passage of air during respiration through the openings in the diaphragm—the air entering primarily the lung cavity through the external wound. The fact that the air did not pass in and out of the external wound during the act of respiration afforded good evidence that the lung was not wounded. The wounds of liver were of such character as to make little or no impression beyond what might occur as the result of injury to the coverings and superficial portions. Puncture of the liver with a trocar is infrequently performed with a view of evacuating fluids. Instances are reported in which no fluid has been found, and no harm has been inflicted by the tapping. Extensive laceration, the result of gunshot wounds or rupture following falls, produce characteristic symptoms of shock and internal hemorrhage.

SECONDARY SARCOMA OF HEART, LUNGS, AND GALL-BLADDER, FOLLOWING PRIMARY AMPUTATION FOR DEPOSIT IN THE FEMUR.

DR. WILLARD exhibited the heart, lungs, and gall-bladder of a female aged 21, whose right thigh had been amputated four months previously for a spindle-celled sarcoma of the lower end of the femur. The apex of the right ventricle was infiltrated with a sarcomatous mass which extended into a cavity among the columnæ carneæ, forming an irregular shaped body occupying one-fifth of the space. The walls were softened. The diseased tissue was very soft and easily detachable, rendering its propulsion into the lungs a matter of exceeding probability at each heart-beat. The walls above the mass were natural in appearance and in thickness; the valves showed no evidence of disease on either side of the heart. The left ventricular and both auricular walls were healthy. The disease had not reached the visceral layer of the pericardium, and there was no abnormal effusion in the cavity of the sac. The septum ventricularum was not involved. That numerous particles had been swept into the lungs was very evident when these organs were examined. At a large number of points in either lung were to be seen white masses, varying in size from that of a pin's head to that of an English walnut. Some of these were dense, others were undergoing softening, and in nearly every instance the lung substance surrounding was so disintegrated that the mass seemed to be in a cavity containing a drachm or more of sanguinolent fluid. A very moderate degree of pressure would cause a nodule near the surface to burst its pleural covering and give rise to an accident similar to the one which was found to have occurred near the right apex. At this point a large sarcomatous mass

had excited a degree of inflammation sufficient to fasten the lung to the parietal pleura, and one week before the patient's death, ulcerating through the serous covering, had given rise to an internal hemorrhage that was well-nigh fatal, and gave the symptoms of sudden collapse noted in the history. This escaped blood was found in the right pleural cavity confined by adhesions chiefly to the upper portion of the chest. In the week which elapsed between the hemorrhage and death, it had coagulated, formed chicken-fat clots and other coagula weighing fully two pounds. The pleural cavity below the adhesions contained about two quarts of bloody serum. There was no consolidation of the lungs save around the diseased foci. The lungs had evidently acted as a complete strainer and had prevented the passage of emboli, for liver, kidneys, spleen, and all other organs were healthy save one small spot in the gall-bladder. The brain was not examined. The primary disease in the femur had apparently resulted from traumatism, since no difficulty had existed previous to a severe fall upon the knee. From this time the pain on walking was continuous, and four months later there was decided enlargement of the external condyle and swelling in the popliteal space.

The chief points of interest in the case were: *First*, the traumatism acting as an exciting cause. *Second*, that the physician who first saw her detected neither fracture nor luxation, nor anything beyond contusion of the joint. *Third*, the appearance at the end of four months of a pulsating tumor in the popliteal space, which presented a decided bruit, but no thrill. This was due to the lifting of the artery from its bed by the sarcomatous mass. *Fourth*, the non-involvement of the knee-joint, although the nodules had pushed forward the synovial membranes between the condyles posteriorly. The articular cartilage of the femur was intact, although the bone tissue immediately beneath it was extensively diseased. *Fifth*, the return of the disease, not in the stump in the right ventricle, and the failure in circulation and great prostration which came on from four to six weeks after the amputation, and without anything in the condition of the stump to warrant such depression. The patient seemed in *articulo mortis*, yet there was no pain and no dyspnœa, only a feeble rapid heart action accompanied by low delirium and weakness. There were no valve sounds audible. These symptoms were due, as shown post-mortem, to the deposit and development of the sarcomatous mass in the heart. Nature, however, gradually accommodated herself to the new growth, and the patient rallied for a time, so as to be able to walk on crutches, eat heartily, and consider herself in good health. She got fatter, and only slight dyspnœa on exertion, with three or four coughs a day, remained to indicate recurrence of the disease. *Sixth*, a sudden, causeless as to exertion, profuse hemorrhage, from the collapse incident to which she rallied and lived one week, with respiration 30–36, pulse 130-140. *Seventh*, the primary and consecutive growths showed a preponderance of spindle-cells, while the secondary nodules were composed chiefly of round cells. *Eighth*, the post-mortem examination throws great light upon the clinical symptoms, while the great rarity of sarcoma of the heart makes it important to note that there was never any angina pectoris. In Dr. Ingham's report of a case called carcinoma of the heart in the *Transactions* of this Society for 1877, the only case ever presented to this Society, angina pectoris was indicated as one of the diagnostic points. In the report of the Committee on Morbid Growths, Dr. Ingham's specimen was shown to be really an alveolar sarcoma. Secondary sarcomatous growths of the heart are mentioned by various authors, but the histories give no clinical signs of the growth.

DR. BARTON said that in regard to traumatisms causing morbid growths, he considered that they probably had no more causative effect than acting as exciting causes.

DR. FORMAD asked if the exact nature of the primary growth was known; whether it consisted of round or spindle-cells, since it has been stated that the spindle-celled variety never form metastasis.

DR. SEILER replied that the primary growth consisted of both round and spindle-cells.

DR. SHAKESPEARE agreed with Dr. Barton as to the origin of the primary growth. He did not believe that injuries were anything more than exciting causes in those predisposed to such growths. The case presented a typical example of the method of metastasis; we have the growth first developing at the knee, whence particles were carried by the veins to the heart, becoming there lodged, and developing into a tumor, which formed a new centre from which microscopic emboli were carried by the blood-current into the lungs, where they lodged and grew into the nodules seen in the specimen. Metastasis of sarcoma occurs by means of the blood-current, while that of carcinoma takes place through the lymphatics.

DR. FORMAD could see no other cause for the tumor than the injury of the knee, previous to which the patient had never shown any symptoms of disease of the joint, while shortly after receiving the injury the tumor appeared. The tumor may not necessarily be malignant; there is an inflammation and the formation of cells; the malignancy will depend upon the looseness of the cells, and the facility with which they can be transported.

DR. L. R. MILLS then read a paper on the *Brain in Epilepsy*.

DR. BRUBAKER presented a specimen of *Tumor of the Brain*, which, with the accompanying paper, was referred to the Committee on Morbid Growths.

CORRESPONDENCE.

COLOR-BLINDNESS.

To the Editor of THE MEDICAL NEWS.

SIR: On page 712 of your December 23d number you have quoted a notice from the *British Med. Journ.* of November 16th, in relation to the examinations of Dr. Kolbe and other Russian physicians for color-blindness. The article speaks of the very great variation in the percentage found by different observers among railroad employés, varying from 0.85 to 5.00. It does not, however, include Dr. Kolbe's explanation, which he gives in reporting the Russian statistics in the *St. Petersburgh Med. Wochenschrift*, October 23, 1882. This is important for us just commencing the testing on the railroads of this country. It seems that the three gentlemen who found so few color-blind were "sold" by the defective employés getting some normal-eyed to personate them during the examination. Dr. Kolbe wrote me that he had heard this from these gentlemen, and as it has now been proved he publishes it. Thus all criticism as to extraordinary variation in percentage, and consequent doubt as to the observer's skill or the reliability of the method used, falls to the ground. The great value of Holmgren's test is shown by the very uniform results experienced and competent examiners obtain by its use. It and all other methods are useless in the hands of the laity or the inexperienced physician.

Respectfully yours,

B. JOY JEFFRIES, M.D.

BOSTON, 15 CHESTNUT ST.,
December 30, 1882.

POLYGONUM HYDROPIPEROIDES.

To the Editor of THE MEDICAL NEWS.

SIR: In the MEDICAL NEWS of December 9, I find a short notice of *Polygonum hydropiperoides*, as given in Eberle's *Materia Medica*.

Dr. Eberle says it was brought to his notice by a country practitioner who made it his thesis subject. This was while Dr. Eberle was Professor of Theory and Practice in the Medical College of Ohio, and the country doctor was one Bud Eastman, then practising in the village of Paris, Jennings County, Indiana.

The writer was personally acquainted with Dr. Eastman, and has frequently heard the doctor speak of his thesis upon "*smart weed*." If there is any credit due any one for its introduction into our therapeutics, it is to Dr. Eastman, who was a man of more than ordinary ability in the profession—a safe, careful practitioner. He finally fell in with Millerism or second adventism, became a noted lecturer, drifted south, and died in Texas some twenty years ago.

The writer has practised medicine thirty-three years, and during this time has prescribed the *Polygonum* many hundred times, and has ever found it an emmenagogue of rare virtue. J. W. CONWAY, M.D.

MADISON, IND., December 15, 1882.

A CARD FROM MESSRS. W. H. SCHIEFFELIN & CO., OF NEW YORK.

To the Editor of THE MEDICAL NEWS:

SIR: In your issue of Dec. 16, 1882, you publish an article giving the results obtained by "an expert analytical chemist" (name and fame to us unknown) in the analysis of quinine pills.

We wish it distinctly understood, at the outset, that we are in favor of a most rigid examination of all chemical and pharmaceutical productions, whether of our make or of others, and we do not doubt that your wish is to do justice to all. But we assert that the value of analyses of complex organic mixtures, like pills, depends very largely upon the previous training and experience, as well as the good name and reputation, of the analyst. An analysis not accompanied by the name of the analyst as a pledge of good faith, has no more value than has an unsigned bank check. It is worthless.

Before criticising in detail the methods and results upon which you base your charge, that we, among others, are either careless or dishonest, and are furnishing the trade with short weight quinine pills, we wish to state that *the charge is not true, so far at least as we are concerned*, and we have positive proof to substantiate our statement.

We ask now that we may be allowed to correct some of the erroneous statements which appear in your article, as well as to record some of our objections to the methods of analysis. For convenience of reference we will classify our remarks.

I. A FAULTY METHOD OF ANALYSIS.

Twenty pills were taken for analysis. They should contain 40 grains of sulphate of quinine $(C_{20}H_{24}N_2O_2)_2.H_2SO_4.7H_2O$, equivalent to 29.725 grains of anhydrous quinine $(C_{20}H_{24}N_2O_2)$, or 1.486 grain per pill of anhydrous quinine. The analyst recovered 1.420 and 1.424 (average, 1.422) grain of anhydrous quinine from each of our pills; he failed to recover 0.064 grain of anhydrous quinine from each of our pills, a quantity equivalent to 0.086 grain of sulphate of quinine. Failing, therefore, to recover 1.28 grain of anhydrous quinine in a total amount (in 20 pills) of 29.725 grains, you see fit to charge us with not putting in the full amount. In reply we would say that *the method of assay which was used is not one which will allow all the quinine present to be recovered.*

1. *The use of excess of ammonia as the precipitant was unfortunate.* It has been repeatedly shown that quinine is nearly insoluble in solution of soda, more soluble in solution of potash, and very soluble in ammonia. Because of this greater solubility in ammonia the Pharmacopœia (p. 160) directs the use of solution of soda in the assay process for quinine in the scaled salt, *Ferri et Quininæ Citras*. Again, Kerner's test for the purity of sulphate of quinine, now prescribed by the U. S. Pharmacopœia (p. 279), depends upon this very solubility of quinine in ammonia. It is certainly very difficult, if not practically impossible, to remove quinine completely from a solution containing excess of ammonia. In this method of "shaking out" an alkaloid by means of some solvent, the wider the differences in solvent power of the liquids present the more complete the extraction; hence soda would have been a very much better precipitant than ammonia. This statement is corroborated by our own frequently repeated laboratory assays of the scales referred to above.

2. *The use of ether as a separative solvent was also unfortunate.* In selecting a solvent for "shaking out" methods of assay two points must be carefully guarded.

a. The substance should be very much more soluble in the new solvent than in the alkaline liquid from which it is to be extracted.

b. The new volatile solvent should itself be as nearly insoluble as possible in the alkaline liquid.

Neither of these conditions is fulfilled by ether. Pure stronger ether is soluble, according to Dr. Squibb, in eight times its volume of water. Therefore, in any "shake method" of assay, the lower aqueous layer will surely be fully saturated with ether, which latter, in turn, will hold in solution a very perceptible amount of quinine. For this reason many experienced analysts, and notably the U. S. Pharmacopœia (p. 160), favor the use of chloroform.

For the extraction of quinine from uncomplicated aqueous solutions containing excess of soda, no solvent which we have used can compare with chloroform; it is a better solvent for quinine than is ether, and is more nearly insoluble in alkaline solutions (see articles Æther Fortior, Chloroformum Purificatum, and Quinina, U. S. Pharmacopœia, 1880).

3. *The test with potassio-mercuric iodide, as here described, could not fail to give a false indication as to extraction of the quinine.* Quinine is precipitated by this reagent only in neutral or acid solutions; the precipitate so produced is soluble in ammonia. This test, therefore, applied to the "remaining aqueous" (ammoniacal) "liquid" is absolutely worthless as regards the quinine present. On the other hand, gelatine, if present, could hardly fail to be precipitated by Mayer's solution whether quinine were present or not. (*Vide* Prescott's *Proximate Organic Analysis*, pp. 139, 140.)

4. *No allowance is made for experimental errors.* We have shown that the analytical process is one subject to several serious errors, and that, with the most conscientious attention to details of manipulation, the analyst cannot fail to leave unextracted a notable quantity of quinine.

A proper regard for the rights of the parties accused should have prompted the analyst to determine exactly the limits of error to which his process was subject. We can only account for this oversight upon the supposition that the difficulty of extracting quinine from complex mixtures was not fully appreciated by the "expert" employed.

II. MATHEMATICAL ERRORS.

You have seen fit to condemn various firms, and, in our case, the analyst's failure to extract $\frac{8}{100}$ grain of quinine from each *two*-grain pill, is his reason for charging us with dishonesty or inexcusable negligence.

In matters of this importance, where every $\frac{1}{100}$ grain has great weight in determining our honesty or dishonesty, we have a right to demand that simple mathematical calculations shall be correct. And if we shall show that the analyst has made several errors, we have a right to infer that he himself has been careless.

We beg to point out the following errors:

Manufacturers.	Series.	Analysis.	Anhydrous Quinine.	Analyst's Equivalent Sulphate of Quinine.	Correct Equival-nt Sulphate of Quinine.	Analyst's Error.
W. H. Schieffelin & Co.,	I	VII	1.42	1.91	1.91	0.00
W. H. Schieffelin & Co.,	II	III	1.424	1.91	1.92	0.01—
Keasbey & Mattison,	III	II	1.42	1.92	1.91	0.01+

It is very peculiar that 1.42 grain of anhydrous quinine should in one case be equivalent to 1.91 grain of sulphate of quinine, and in another case to 1.92 grain, while 1.424 grain of anhydrous quinine represents only 1.91 grain of sulphate of quinine.

We do not care to quibble over $\frac{1}{100}$ grain, and we would not, were it not that we are judged by hundredths-grains, but we refer to the above to show that the *analyst has been careless in making calculations.*

III. MISLEADING COMPARISONS.

We wish to call attention to a singular and very unfair comparison of one sample of pills containing bisulphate of quinine with six samples containing sulphate of quinine.

In speaking of "quinine pills" it is always, and very properly, understood that the pills contain the ordinary medicinal sulphate $(C_{20}H_{24}N_2O_2)_2.H_2SO_4.7H_2O$, a salt containing 74.31 per cent. of anhydrous quinine. Bisulphate of quinine is quite another salt, as appears from its chemical formula, $C_{20}H_{24}N_2O_2.H_2SO_4.7H_2O$.

Now, bisulphate of quinine contains only 59.12 per cent. of anhydrous quinine, and the injustice of your comparison appears in this, that, to those not chemists, you apparently affirm that 1.24 grain of anhydrous quinine represents more "sulphate of quinine" than does 1.42 grain of anhydrous quinine (Table, p. 690). 1.24 grain of anhydrous quinine is in reality equivalent to 1.67 grain of ordinary sulphate of quinine, as appears from the following proportion:

$$74.31 : 100 = 1.24 : x \qquad x = 1.67$$

In your conclusions (I., p. 691) you speak of seven samples of pills, five of which "*have not in them the amount of* SULPHATE OF QUININE *which they are represented to contain.*" Any competent chemist knows that a comparison of pills containing salicylate of quinine with those containing the ordinary sulphate would be no more unfair and misleading than the comparison of bisulphate with sulphate.

And now, it is due to our friends in the drug trade who have so unhesitatingly recommended our pills to the physicians, and due also to the great number of the medical profession who have become accustomed to administer them with implicit confidence, that *we repudiate the imputation which has been put upon us of either carelessness or dishonesty.* We do deny emphatically that the pills tested were short weight. We have shown how the analysis might have failed in finding a true result from imperfections in its method. We now assert that the analyst was at fault in regard to our pills.

1st. Because *we know* that the full weight of sulphate of quinine was put in them.

2d. Because, since the publication of your article, we have examined the record of our production of them as far back as the 1st of July, and find that the variation between the quantity of sulphate of quinine used, and the corresponding number of pills produced amounts to a mere fraction of one per cent., a degree of accuracy unattainable in extemporaneous dispensing. Pills made to order on physicians' prescriptions can be divided with only approximate exactness, and to make correctly only a few granules at a time in minute subdivisions is an impossibility. With us the liability to error is reduced to a minimum, for the chances of mistake in one skilled person making 5,000 pills at once, as against one hundred persons making collectively the same number, are as one to one hundred.

3. We have repeatedly had them analyzed by Mr. H. B. Parsons, who is not unknown as a chemist, and they were found by him to contain sulphate of quinine in full weight.

In regard to the comparison of prices, we care nothing, and only refer to it in order to show where the compiler seems to be weak, and that is in exactness or in carefulness. A slight examination of the table will show that, while purporting to give retail prices, it has, in some cases, given wholesale long prices, and in others wholesale net prices, and is as misleading as are the analyses.

In conclusion we desire to call your attention to the fact that the injury which such a publication as the one under consideration is calculated to inflict on parties whose names are used in such a connection is not likely to be overcome by any reply which may be made, as in many cases the persons who may have read the first article will never see the reply, and, if they do, will perhaps not read it carefully; we trust, therefore, that your sense of justice will induce you to give this reply as general and wide a circulation as that accorded the original article.

Yours truly,

W. H. SCHIEFFELIN & CO.

NEW YORK, December 30, 1882.

[In reply to the above card, it seems almost unnecessary to state that the report of the results of our analyses of quinine pills was not put forth anonymously, but was published as the work of THE MEDICAL NEWS Commission, and "the good name and reputation" of THE MEDICAL NEWS is the guarantee of the trustworthiness of the results obtained.

It is unfortunate that Messrs. Schieffelin & Co.'s general denial of the accuracy of our analyses is not accompanied with that "positive proof" which they assume to furnish. Apart from a general assertion as to the quantity of quinine issued since July 1st—an assertion which we of course do not presume to question—their card resolves itself, when examined, into criticisms reflecting upon the analysis, and objections to the method employed. The several points of objection will, therefore, be considered *seriatim.*

As regards the statement that "the method of analysis was faulty" because: 1. "The use of an excess of ammonia as the precipitant was unfortunate." 2. "The use of ether as a separative solvent was also unfortunate;" the simple fact is that, by this means, the amount of quinine contained in a solution may practically be *fully* recovered.

That quinine is more freely soluble in ammoniacal liquids than in pure water, or in solutions of soda or potassa, and that it is more freely soluble in chloroform than in ether, are facts which were considered to be so well known as to render any allusions to them unnecessary.

That ether is soluble in water to the extent mentioned was not overlooked in the making of our analysis. It is, however, plainly evident that the amount of quinine held in solution by the ether dissolved in the water must of necessity be reduced to an inappreciable amount, when the relative volumes of the aqueous liquid and ether employed are considered, and when the aqueous liquid is repeatedly shaken with fresh portions of ether and the ethereal layer in each instance is carefully separated. That the quinine is thus as completely recovered as is possible by any method of assay, is believed to be satisfactorily proven by the application of the test with potassio-mercuric iodide.

To the statement that "the test with potassio-mercuric iodide, as here described, could not fail to give a false indication as to extraction of the quinine," the answer is that the conditions attending the application of this reagent as a test for alkaloids were perfectly well known to our Commission, and the statement was not made in the report that the *ammoniacal* liquid was directly subjected to the test. The "remaining aqueous liquid," as stated in our report, was, indeed, tested; but not, however, without having been previously supersaturated with an acid; a condition which was considered so well known to chemists as not to require specific description.

The Messrs. Schieffelin further state that "for the extraction of quinine from uncomplicated aqueous solutions containing excess of soda, no solvent which we have used can compare with chloroform; it is a better solvent for quinine than is ether," etc. etc.; and further, the supposition is expressed "that the difficulty of extracting quinine from complex mixtures was not fully appreciated by the 'expert' employed." Now, it must be conceded that an aqueous solution of quinine containing considerable amounts of sugar, gelatine, etc., can *not* be considered an "uncomplicated solution," and that an amount of chloroform which would be sufficient readily to abstract the quinine from a pure solution, or one containing simple inorganic salts, would form, in the case of a solution thus complicated, a very viscid mixture.

The exactitude of the methods, and the avoidance of what Messrs. Schieffelin & Co. call "experimental errors," are sufficiently proved by the close coincidence of results arrived at in successive analyses, without any knowledge on the part of the operator as to the source of the specimens on which he was at work.

As considerable prominence has been given by Messrs. Schieffelin & Co. to the subject of "mathematical errors," and as they "demand" that "the mathematical calculations shall be correct," we presume that they will be satisfied when they see that such has been the case.

The discrepancy of $\frac{1}{100}$ grain in the tabulation of the result, which they supposed they had discovered, is attributable to the fact that in the second series of analyses, in stating the amount of anhydrous quinine, the third decimal was inserted, which was omitted in those of Series I. and III. The amounts of anhydrous quinine obtained by our Commission, extended to the third decimal, were as follows:

Manufacturers.	Series.	Analysis.	Anhydrous Quinine.	Equivalent of Sulphate of Quinine.
W. H. Schieffelin & Co.....	I	VII	1 422	1.814
W. H. Schieffelin & Co.....	II	III	1.424	1.816
Keasbey & Mattison.....	III	II	1.426	1.919

The statement criticised was, therefore, as correct as was possible within two decimals.

The Messrs. Schieffelin & Co. call attention to what they consider "a singular and very unfair comparison between bisulphate and sulphate of quinine pills," which, as stated by them, would mislead the reader into supposing that we had compared the amount of anhydrous quinine in these respective pills. It seems almost unnecessary for us to deny that we made any such comparison. The investigation by our Commission was solely to ascertain if the quinine pills on the market contained the amount of quinine sulphate, or bisulphate, as the case may be, which they were represented to contain, and the clear view of the result is not to be clouded by any extraneous issue. The reason why in the case of one house the bisulphate pills were analyzed was because that house sells and advertises to sell the bisulphate whenever quinine pills are called for, since they are believed to be more readily soluble than the sulphate.

As regards the comparison of prices, it is sufficient to remark that the Commission simply reported the prices which it paid, like any other retail purchaser. This is all that the profession and their patients are interested in; the questions of "wholesale long prices," and "wholesale net prices," are trade mysteries with which THE NEWS has nothing to do.—ED.]

NEWS ITEMS.

BOSTON.

(From our Special Correspondent.)

DR. CARPENTER'S SIXTH LECTURE ON HUMAN AUTOMATISM.—This lecture, which concluded the course, considered automatism in morals. The lecturer began by stating the theory of pure automatism, as distinguished from his own theory of automatism controlled by the will, as follows: Man cannot be an exception to the general uniformity of nature; all his action is the result of what has gone before; choice is as absurd as in the case of a bar of iron between two magnets; "I cannot be other than I am; I cannot do otherwise than I do," is the automatist's creed. This position was then criticised as not taking account of human nature, which does not work in sequences as material forces do. The theory of a limited choice on the part of the human agent will explain all the facts brought forward by the automatists and many which their theory will not explain. A large body of moral experiences is utterly inconsistent with the automatist view. Now moral experience is determined by the original constitution and acquired habits. The habits are formed in youth, and the most important part of early training is that which relates to control of the bodily appetites and passions, as well as of the intellectual powers. As a proof of the freedom of the human agent, the lecturer dwelt upon the power of fixing the attention. He next considered the several motives which excite human action. First of all is the desire for air, which is so intense that persons never take their own lives by suspension of the breath, but by direct act of the will. But they overcome this desire by a flank movement, by means of muscles over which the will has more powerful control. Thus individuals commit suicide by holding the face in a basin of water, or by hanging when they could easily save themselves by touching the feet to the floor. Next ranks the desire for food. The influence of alcoholic drink was discussed, the lecturer saying that alcohol seriously modifies the constitution and its effect is hereditary.

Coleridge was cited as the most remarkable example of intellectual antomatism of the brightest type. But he lacked power of self-direction and was put under the control of a guardian. His son Arthur inherited his father's weakness, and was irresponsible for his unhappy end. Moral motives were considered, the dominant motive, according to the lecturer, being the desire to do right. But this motive varies greatly in clearness. In the case of street children, they apparently think that which they like is right, and that which they do not like is wrong. As an instance in point the Feejee Islanders were mentioned. Their morals have been regarded as nearly or wholly lacking, but an experienced observer has found that the Feejees have a strict sense of the weight of the *nomas* or law, and on their plane are rigid in their actions. In regard to morals the lecturer quoted the words which have been said to embody all morals: "I am, I ought, I can, I will." The first implies power of introspection. The second directs our attention to the consequences of our acts, and leads us to intensify their force by keeping the mind upon them. We possess the power of self-regulation in the power of control over our fixed character. By fixing our minds upon a lofty motive a flank movement may be made which shall defeat a base motive. No human power can be so strengthened by practice as this power of self-control. Every successful moral conflict leaves the victor so much the stronger—with so much the more good—for the next struggle. The best means to conquer temptation is to fix the mind upon some other object. It is bad policy constantly to say. "I will not yield to this," for then the object is kept before the mind; but the mind should resolutely be turned to some other thing. Instead of brooding over wrongs and the slights inflicted upon us by friends, the remedy is not to say: "I will not think of it," but to direct the mind into some other channel. Morose feelings, frequently, are the result of some bodily derangement, often of the liver. Nothing is worse than brooding. In periods of depression the lecturer had found much relief in reading Scott's novels. He concluded by saying that the moral automatism is largely under the control of the will.

THE APPROPRIATION TO THE ARMY MEDICAL MUSEUM AND LIBRARY.—We are glad to be able to announce that the House of Representatives, in the debate on the Army Appropriation Bill, has restored the appropriation for the Army Medical Museum and Library to the usual amount. The following is an extract from the *Congressional Record* of Jan. 4th:

MR. BUTTERWORTH.—In regard to that item, I will state to the Committee that it was reduced by the subcommittee on Appropriations from $10,000 to $7,500, and we had such a storm raised about our ears because of the fact that this is the pet of the whole medical profession, that after we had reduced it to that sum I moved to restore it to the amount of the estimate. The Committee on Appropriations, the whole committee, instructed, however, that this should be restored to $5,000. It has heretofore been $10,000. It relates to the collections made during the war, and stored in the Army Medical Museum, and for works of reference for the Surgeon-General's office; and I believe it is the finest collection possibly in the world. They insist that a larger sum is necessary. How it is necessary, however, was not made to appear, and the Committee instructed this reduction to be made.

MR. TOWNSHEND, of Illinois.—Let me ask the gentleman, if any consideration was given to the construction of a fire-proof building for the museum?

MR. BUTTERWORTH.—Yes. I will say to my friend this, that a part of the public building proposed to be erected for a library, would be devoted exclusively to this museum, and to the articles there gathered, together with the library.

MR. TOWNSHEND, of Illinois.—I have received communications from many persons, who speak of the library and museum as of great value, and of the necessity for the erection of a fire-proof building for their preservation.

MR. BUTTERWORTH.—There is no doubt of it. The gentleman is correct. It is asserted by many doctors to be one of the most valuable collections in the world; and it is now stored in what you might almost say is a mere tinder-box, liable to be destroyed by fire at any time. That is one of the reasons the committee deemed it unnecessary to add materially to the collection while it is in that building, but deemed it proper to withhold appropriations until a more suitable building was secured.

MR. TOWNSHEND, of Illinois.—Do the committee make any recommendations upon the subject?

MR. BUTTERWORTH.—The committee believe that a part of the new library building might be necessary for this museum.

MR. TOWNSHEND, of Illinois.—Does the gentleman think that it would be wise to separate the library from the museum? In my judgment they ought to be together.

MR. BUTTERWORTH.—The proposition was, to set apart a section in the new library building, to be devoted to the museum and its library.

MR. HEWITT, of New York.—Permit me to say that the amount, $10,000, appropriated last year was not, in the judgment of many eminent in the medical profession, sufficient. That, I suppose, the gentleman from Ohio understands. Five thousand dollars is entirely inadequate. This library is the most complete library in the world on medical subjects. The object is to complete it, and to keep it complete. Five thousand dollars a year will not purchase the new publications necessary to keep it up to its present standard. I think this is perhaps the wisest expenditure of money we can possibly make with reference to disease.

MR. BUTTERWORTH.—If the gentleman from New York will yield to me, I beg to state that I will move to increase the appropriation to $10,000.

MR. HEWITT, of New York.—That is right.

MR. TOWNSHEND, of Illinois.—I desire to say a word, unless the gentleman from New York (Mr. Hewitt) wishes to retain the floor.

MR. HEWITT, of New York.—I have finished what I had to say.

MR. TOWNSHEND, of Illinois.—I think true economy would require we should make provision for the preservation of the library and museum, and that a mere increase of the appropriation is not sufficient. I would suggest to the gentleman from New York (Mr. Hewitt) or the gentleman from Ohio (Mr. Butterworth) to bring in a provision here for the erection of a fire-proof building for the preservation of the museum and library.

MR. BUTTERWORTH.—I do not think that would be proper in this bill, although I agree with my friend from Illinois it is something which ought to be done.

The question being taken on the amendment offered by Mr. Butterworth, to strike out "$5,000," and insert "$10,000," it was agreed to.

CITIZENS' AUXILIARY SANITARY ASSOCIATION OF NEW ORLEANS.—According to the New Orleans papers a sanitary war is in progress in that city. It appears that the Auxiliary Sanitary Association, which was called into being by the sufferings of the city during the epidemic of 1878, and which since that time has labored to improve the sanitary condition by securing

clean streets, and the removal of all nuisances which are so apt to accumulate in an unsewered city, has recently made an effort to prevent the spread of smallpox by representing to the City Council the danger attending upon the usual burial service in cases of death from this disease. The association in the first instance offered to coöperate with the board of health to obtain appropriate legislation, but it does not appear that any action was taken by that body to meet the views of the citizens. One gentleman complained that a fatal case of smallpox occurred in the house which adjoined his own, and that no warning was given by the board of health. His first knowledge of the existence of the disease so near to his own household was conveyed by the funeral of the deceased. At a meeting of the board, a few days later, it decided that the association, instead of remaining "auxiliary," was becoming "*supersidiary*," and forthwith passed a resolution for its suppression. Nevertheless the association yet lives, and has tendered its support in the work of sanitation to the City Councilmen in an address, in which it adverts to some of the most important permanent works which have been accomplished by it.

SMALLPOX IN VIRGINIA.—A telegraphic despatch from Lynchburg states that smallpox has made its appearance at Salem, Roanoke County, and that thirty cases have already been reported. The neighboring towns of Roanoke and Wythville have quarantined against Salem, and other southwestern towns are considering the question of doing likewise. Roanoke College has suspended, and the students have gone home. The public schools have also closed, and business is at a standstill.

TROUBLE IN MCGILL UNIVERSITY.—A telegram from Montreal states that trouble has arisen in McGill University between the medical students and Dr. Wright, Instructor in Materia Medica. One hundred and forty-seven students demand the resignation of Dr. Wright, and threaten to leave the university if their demand is not complied with.

COMPLIMENTARY DINNER TO PROF. AUSTIN FLINT, SR.—Prof. Flint, who completes to-night his course of lectures before the Philadelphia County Medical Society, was entertained last evening at dinner at the Hotel Bellevue by a number of the prominent members of the medical profession of Philadelphia. Covers were laid for forty, and Prof. Alfred Stillé, President of the College of Physicians, presided, with Prof. Flint on his right and Prof. Gross on his left. Among those who participated were Dr. John L. Atlee, President of the American Medical Association, Drs. D. Hayes Agnew, Da Costa, Goodell, Bartholow, Austin Flint, Jr., S. Weir Mitchell, E. Wilson, A. H. Smith, W. V. Keating, Wm. Thomson, John Ashhurst, Jr., J. H. Brinton, I. M. Hays, and others. The arrangements of the dinner were under the charge of a committee consisting of Drs. Alfred Stillé, J. M. Da Costa, Wm. Pepper, S. W. Gross, James Tyson, Horace Y. Evans, and J. Ewing Mears. Towards the close of the dinner, the chairman proposed, in a few graceful remarks, the health of the distinguished guest, which was responded to by Dr. Flint in a feeling manner.

ELECTION OF OFFICERS OF THE PHILADELPHIA COUNTY MEDICAL SOCIETY.—The annual meeting of the Philadelphia County Medical Society was held on January 3d, and the following officers were elected for the ensuing year:

President.—William M. Welch, M.D.

Vice-Presidents.—Drs. William R. D. Blackwood and Addinell Hewson.

Recording and Reporting Secretary.—Henry Leffman, M.D.

Corresponding Secretary.—H. Augustus Wilson, M.D.

Treasurer.—L. K. Baldwin, M.D.

Librarian.—C. M. Seltzer, M.D.

PROF. HENLE, the eminent anatomist, has been elected, in the place of the late Prof. Wöhler, as permanent secretary of the Royal Academy of Sciences, at Göttingen.

THE NEW YORK SOCIETY OF MEDICAL JURISPRUDENCE.—The newly organized New York Society of Medical Jurisprudence held its first public meeting last Thursday evening at the Academy of Medicine. Dr. J. S. Wight, Professor of Surgery in the Long Island College Hospital read a paper on the "Bearing of Illusions and Hallucinations on Testimony."

This society was organized last month. Its object is the "advancement and study of the science of medical jurisprudence and the attainment of a higher standard of expert testimony." It has a membership of more than one hundred physicians and lawyers. Among the prominent medical members are Drs. Meredith Clymer, T. E. Satterthwaite, Clinton Wagner, T. C. Finnell, C. S. Wood, W. A. Hammond, Nathan Bozeman, T. A. McBride, J. S. Wight, and L. C. Gray. The society will hold monthly meetings.

ABOLITION OF THE GRADUATION THESIS.—The Senate of Toronto University has abolished the statute which requires candidates for the degree of M.D. to write a thesis.

NEW YORK POLYCLINIC.—The attendance at the New York Polyclinic during its first course of six weeks has been such as to assure the success of the institution. Fifty-nine practitioners took out tickets, twenty-seven of these being in the Gynecological Department, and 1089 new patients were treated in the dispensary.

HEALTH IN MICHIGAN.—Reports to the State Board of Health by observers of diseases in different parts of the State, for the week ending Dec. 30, 1882, indicate that intermittent fever has considerably increased, and that scarlet fever, consumption, membranous croup, measles, and remittent fever have increased in area of prevalence.

Compared with the average for the month of December in the preceding five years, neuralgia, tonsillitis, and dysentery were more prevalent, and remittent fever, intermittent fever, diphtheria, bronchitis, consumption, and pneumonia were less prevalent during the month of December, 1882.

Including reports by regular observers, and by others, diphtheria was reported present during the week ending Dec. 30, and since, at 18 places, scarlet fever at 12 places, measles at 7 places, and smallpox at 2 places, as follows: at Richmond, Osceola Co., Dec. 27; at Ionia (one case), Jan. 3, 1883.

Three cases of scarlet fever are reported at Marquette among immigrants just arrived from England. The disease was contracted on the steamer.

OFFICIAL LIST OF CHANGES OF STATIONS AND DUTIES OF OFFICERS OF THE MEDICAL DEPARTMENT, U. S. ARMY, FROM JANUARY 1 TO JANUARY 8, 1883.

MCKEE, J. C., *Surgeon.*—To report, on or before Jan. 1, 1883, to the commanding officer Fort Winfield Scott, Cal., for assignment to duty as Post Surgeon.—*Par. 1, S. O. 197, Department of California, December 28, 1882.*

GORGAS, W. C., *Assistant Surgeon.*—Relieved from the temporary duty to which he was assigned under par. iv., S. O. 137, Department of Texas, and will report to the commanding officer Fort Brown, Texas.—*Par. 2, S. O. 140, Department of Texas, December 26, 1882.*